Immigration Restrictions and the Wages of Low-Skilled Labor: Evidence from the 1920s*

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Abstract

How do immigration restrictions affect the wages of low-skilled workers? We study the 1920s U.S. national-origins quotas, which abruptly and unevenly curtailed immigration from key source countries, generating sharp labor-supply shocks across local markets. Using newly digitized annual wage data for low-skilled laborers from 1910–1929, we show that wages rose faster in markets more dependent on restricted-origin immigrants, with effects emerging soon after 1920, intensifying through the mid-1920s, and persisting over time. Evidence on wage dynamics and mechanisms indicates that these gains reflect sustained labor scarcity rather than mechanical compositional changes.

JEL Codes: J15, J31, K37, N32, N42, R23

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1 Introduction

Current policy debates in the United States and Europe increasingly center on reducing immigration, making it critical to understand the labor-market effects of such policies.[1] Today, as in the past, a prominent argument of those advocating such policies is that they will raise wages for incumbent workers. The nature of the relationship between immigration and wages has been the subject of a significant amount of economic research. However, one feature of almost all empirical studies of the wage effects of immigration is that they analyze either episodes marked by a sudden increase in the number of immigrants or data from periods characterized by high or rising immigration.[2] Evidence from historical cases is sparse: only a few episodes, such as the end of the Bracero program in the U.S., provide direct insight into the wage effects of reduction in immigration, and these largely concern specific sectors such as agriculture rather than the broader economy (Clemens et al., 2018). As a result, we know far less about the wage consequences of immigration restrictions than inflows, even though the two may generate different adjustment dynamics.

This paper examines the most consequential immigration restriction in U.S. history—the 1920s national origins quotas—and provides evidence on how these policies affected the wages of low-skilled workers across local labor markets and over time.[3] To analyze local wage adjustment, we assemble a novel dataset of annual wage reports for laborers in manufacturing and mining, construction, and agriculture from 1910–1929. These data record actual hourly pay rather than occupation-based income proxies commonly used for this period, and their high-frequency structure allows us to observe wage dynamics through the 1920s. We follow the chain-migration logic widely used in the immigration literature (Card, 2001) and construct a measure of how labor supply in each local labor market was likely affected by the quotas ("quota exposure") based on historical immigrant settlement patterns. We then estimate difference-in-differences models with treatment intensity defined by this exposure, building on the empirical approach in Abramitzky et al. (2023).

Our newly assembled and harmonized wage data draw on multiple historical sources spanning 1910 to 1929. The most comprehensive source consists of a series of industry surveys conducted by the Bureau of Labor Statistics (BLS), which report average hourly wages in mining and manufacturing at detailed occupation levels across cities and states, with many industries surveyed repeatedly over time. We complement these data with wage reports for construction laborers from surveys of local building contractors and BLS surveys of union wage scales in various cities, as well as with annual United States Department of Agriculture (USDA) surveys reporting farm laborers' wages at the state level. We also examine post-1920 wage data to determine whether it is consistent with our primary analysis. For this, we use wage estimates from the 1940 full-count census to

[1] Council on Foreign Relations (2024) reviews recent U.S. political debates and rising support for more restrictive immigration policies, while Varma and Roehse (2024) provides an overview of the recent rise in anti-immigration sentiment in Europe and the EU's policy response.

[2] See, among others, Altonji and Card (1991), Card (1990, 2001), Borjas et al. (1997); Borjas (2003, 2017); Borjas and Monras (2017), Dustmann et al. (2017), Hunt (1992), Ottaviano and Peri (2012), and Monras (2020).

[3] The quota laws disproportionately affected the supply of low-skilled workers, as was noted by economists at the time (BLS, 1927) and has been confirmed in several recent studies (Greenwood and Ward, 2015; Massey, 2016; Ward, 2017). We confirm this result in our analysis as well.

examine longer-run wage adjustments and a BLS survey of wages paid in 1928 to workers hired to clean and repair streets in over 2,500 cities and towns.

We find that labor markets more exposed to the quotas experienced a sharp and persistent rise in the wages of low-skilled laborers, along with meaningful declines in employment. These effects appear shortly following the restrictions, strengthen through the mid-1920s, and remain substantial by 1940. For the average labor market in our sample, the observed post-1920 decline in immigration corresponds to a decline in the foreign-born labor share on the order of 3–4 percentage points, accompanied with wage gains on the order of 6–8 percent, consistent with a wage elasticity with respect to foreign-born labor of between -1.5 and -2. The effects are uneven across sectors: manufacturing and mining—industries that had relied heavily on foreign-born labor before the quotas—experience the largest and most persistent wage increases, while agriculture and construction show smaller and more transitory responses.

To guide interpretation, we develop a simple model of labor demand, capital adjustment, and labor mobility across regional labor markets. The model predicts that a negative shock to immigrant labor supply generates an immediate increase in wages for low-skilled workers, followed over time by offsetting adjustments along three margins: internal migration, capital deepening, and industry heterogeneity. The speed and extent of adjustment depend on frictions to labor mobility, industry-level technologies, and the cost of modifying capital stocks. In industries where capital or labor can be adjusted quickly and at low cost, wage effects should dissipate more rapidly, while industries facing high adjustment costs—or that begin with already high capital intensity—should experience more persistent wage gains.

We look for evidence of heterogeneity across labor markets in adjustment dynamics by examining two key adjustment channels: internal migration and capital substitution. Using linked census samples for 1910–1920 and 1920–1930, we track individual laborers over time and find that internal migration did not fully offset the shock. Laborers did not move into high-exposure markets at elevated rates after 1920; instead, these markets exhibit higher worker retention and modest increases in local labor-force entry. We then use data from the Census of Manufactures and the Census of Agriculture to test for capital adjustment. Manufacturing industries with lower pre-1920 mechanization exhibit increases in horsepower per worker post-1920, while highly capital-intensive sectors show little adjustment and the largest wage responses. In agriculture, by contrast, more exposed states adopted machinery more rapidly, consistent with lower adjustment costs in that sector. Taken together, the mechanism tests support the model's assumptions of heterogeneity across labor markets in adjustment dynamics.

A few considerations guide the interpretation of our estimates. Because our wage measures are averages for area–industry cells, part of the observed wage increases could reflect compositional changes if immigrants earned less than natives; Section 7 shows that such effects can explain only a small portion of the estimated wage effects. In addition, the wage patterns we document reflect both the sharp reduction in low-skilled immigrant inflows and the adjustments that followed—including internal migration, shifts in labor-force participation, inflows from unrestricted countries, and industry-specific capital substitution. Our estimates should therefore be understood as relative wage gains in more exposed labor markets after these endogenous responses. Since such labor and capital adjustments would accompany any sizable decline in immigration, the medium-term effects we document are more informative about the likely consequences of immigration restrictions than estimates of outcomes absent adjustment.

Related Literature. Our analysis contributes to research on the labor-market effects of immigration restrictions. The closest parallel is Clemens et al. (2018), who show that ending the Bracero program had little impact on U.S. farm wages because farmers rapidly substituted toward machinery and less labor intensive crops. Modern U.S. and European studies similarly find that restrictions and enforcement shape labor-market outcomes: tighter U.S. border controls raised relative wages in border regions (Hanson and Spilimbergo, 1999), IRCA sanctions altered labor supply and employment patterns (Orrenius and Zavodny, 2003), and the wage effects of immigration in Europe are larger where labor-market institutions limit adjustment (Angrist and Kugler, 2003). These studies highlight how labor and capital responses mediate the effects of immigration restrictions, but they offer limited evidence on large economy-wide declines in immigration.

A related insight from the literature is that wage responses to immigration shocks need not be symmetric for inflows and restrictions, because the underlying adjustment margins may differ. While standard models predict that wage effects dissipate as capital and labor adjust, empirical evidence suggests that capital adjustment following labor scarcity may be slower and more uneven across industries than adjustment to labor abundance (Cooper, 2006; Pindyck, 1991). Labor mobility may also respond asymmetrically: native outmigration following wage declines can be reinforced by both economic and non-economic factors, whereas in-migration in response to rising wages may be constrained by mobility costs and institutional frictions (Greenwood, 1997; Ferrie, 1997; Collins and Wanamaker, 2015; Boustan, 2010; Card, 2005).

Our paper also relates to work examining how heterogeneity in capital intensity and capital-adjustment costs across industries shapes wage responses to labor-supply shocks. In postwar agriculture, studied by Clemens et al. (2018), mechanized technologies were already widely diffused and capital services readily available, facilitating rapid substitution and fast wage convergence; similar forces operate in other modern settings (Lewis, 2011; Hornbeck, 2012). By contrast, the industries we study in the 1920s differ sharply in both their reliance on immigrant labor and their scope for capital substitution. This cross-sector heterogeneity allows us to show that wage responses are largest and most persistent in industries where capital adjustment is slowest, providing direct evidence that differences in production technologies and adjustment frictions are central to understanding the incidence and persistence of wage effects following immigration restrictions.

Our paper is also closely related to Abramitzky et al. (2023), who provide the first comprehensive analysis of the aggregate labor-market consequences of the 1920s national-origins quotas using census data and occupation-based income measures. They document large declines in immigrant inflows, substantial replacement by unrestricted natives and immigrants, and important adjustments in production technologies, but find limited wage effects for the average worker in the labor market. We complement and extend their analysis by focusing on the segment of the labor market most directly exposed to the quotas—low-skilled laborers—and by using newly assembled data on actual hourly wages rather than imputed occupational-based income measures.[4] This distinction is central: occupation-based income measures mask within-occupation wage adjustment, while average wage measures such as those provided by the Censuses of Manufacturing aggregate across heterogeneous workers. By isolating narrowly defined laborer occupations and tracking their wages at high frequency, we uncover substantial and persistent wage gains in highly exposed markets that are not visible in average-worker outcomes. In addition, while Abramitzky et al. (2023) emphasize internal migration and capital deepening as key margins of adjustment at the aggregate level, our results show that for laborers—the workers most directly affected by the contraction in immigrant labor—migration responses are more limited and industries differ in the extent and speed of capital adjustment.

Our work is also related to studies on the 1920s quotas by Price et al. (2020) and Tabellini (2020). These papers use identification strategies similar to ours and generally find muted wage effects using decennial census data and occupation-based income measures. We revisit these conclusions using wage measures that are closely aligned with the workers most exposed to the quotas and that allow us to observe short- and medium-run dynamics at annual frequency. More broadly, our study connects to research examining how the end of mass European immigration reshaped U.S. labor markets and economic development. This work shows that the quotas altered immigrant selection and return migration (Greenwood and Ward, 2015; Massey, 2016; Ward, 2017), contributed to changes in Black migration patterns (Collins, 1997), reduced mortality rates (Ager et al., 2024), reduced scientific innovation (Moser et al., 2025), and affected public attitudes toward redistribution (Tabellini, 2020). We build on this literature by providing systematic evidence on how the quotas affected low-skilled wages across multiple industries and local labor markets.

Last, our findings speak to the extensive literature on the effects of immigration on wages. A large body of work examines how immigration inflows affect local labor markets across education groups, experience cells, and cities with mixed conclusions. Two themes from this literature are especially relevant to our setting: (i) wage effects tend to be largest among workers most similar to immigrants (Borjas, 2017), motivating our focus on low-skilled laborers; and (ii) adjustment dynamics unfold over different time horizons (Dustmann et al., 2017; Monras, 2020), underscoring the importance of the high-frequency wage data we assemble. Our contribution is to bring these insights to bear on the largest immigration restriction in U.S. history, using new data that allow us to trace both short-run adjustments and medium-run persistence.

[4] Abramitzky et al. (2023) also examine manufacturing wages, but these measures combine production workers of all skill levels, making them less sensitive to wage changes concentrated among the lowest-skilled.

2 Historical Background

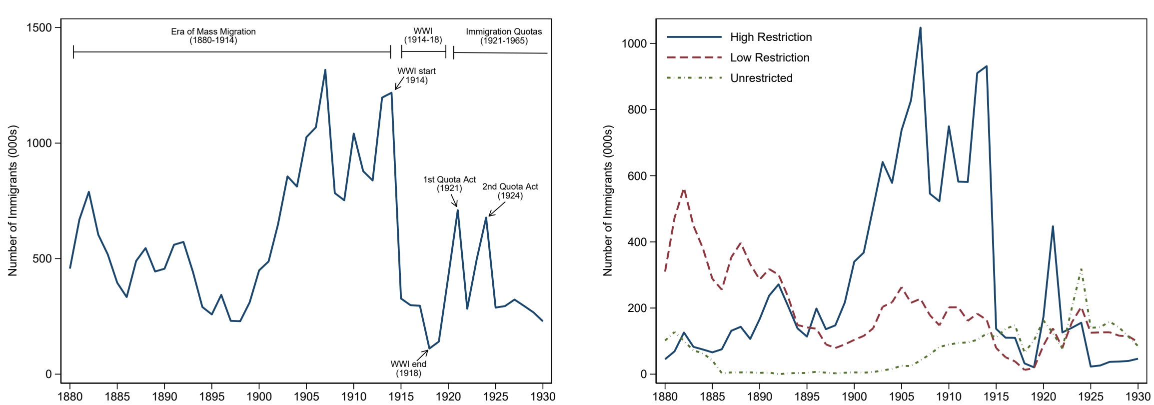
At the start of the twentieth century, the United States was the world's leading destination for migrants. Between 1900 and 1914, more than 13 million immigrants arrived, most from Southern and Eastern Europe, providing much of the labor for expanding industries in cities like New York, Chicago, and Pittsburgh (Goldin, 1994; Higham, 2002). This "new immigration" transformed the country's workforce and provoked a long-standing debate over the social and economic consequences of large-scale migration.[5] Similar to nowadays, reformers, labor leaders, and policymakers disagreed over whether immigration depressed native wages or instead fueled economic growth.[6]

Panel (a) of Figure 1 shows the dramatic changes in immigration flows around the 1920s. During the World War I, transatlantic migration nearly came to a halt as ships were diverted to military use and European governments imposed passport and travel restrictions. After the war, migration rebounded: by 1920, arrivals again exceeded half a million per year, renewing public anxiety about the scale of immigration. Congress responded first with the Emergency Quota Act of 1921, which capped arrivals from each country at 3 percent of the number of immigrants from that country in the US as of the 1910 Census, and then with the Immigration Act of 1924, which reduced the percentage to 2 percent and shifted the base year to 1890. These laws marked the beginning of a new restrictive immigration regime that would define U.S. policy for the next four decades.

Figure 1. Immigration Flows by Quota Restriction Category

(a) Immigration Flows - Timeline

(b) Annual Flows - By Restriction Category



Source: Historical Statistics of the United States: Colonial Times to 1957, Series C-88-116. Series C 88-114. "Immigrants, by Country: 1820 to 1957".

Note: See Table A1 for a list of countries/regions and their classification.

The laws led to a drastic change in the composition of immigration flows in terms of the nation of origin of the immigrants. This is shown in panel (b) of Figure 1, which follows Abramitzky et al. (2023) in dividing immigrants to the U.S. into three categories: those from the "high restriction"

[5]This contrasted with the "old immigration" from northwestern Europe and the British Isles.

[6]Some examples include Commons (1907), Jenks and Lauck (1911), and USIC (1911) .

countries of Asia and of Southern, Eastern and Central Europe (countries for which the quotas introduced in the 1920s were generally binding); those from "low restriction" countries of Northern and Western Europe (for which quotas often did not bind); and those from western hemisphere nations, which were not restricted by the Acts. As the figure shows, immigration during the pre-war period (1910-1914) was dominated by the new immigration. The imposition of quotas in 1921 reduced the new immigration, while the unrestricted immigration from the western hemisphere (mainly from Canada and Mexico) rose above its pre-war level, and immigration from the low-restriction countries came close to pre-war levels. Finally, following the 1924 Act, immigration from the high-restriction countries dropped below 50,000 per year.

During the years following the legislative imposition of quotas, there was a broad consensus among economists that limitations on immigration were indeed placing upward pressure on wages. (see, e.g., Berridge (1923), Douglas (1926), and Hansen (1923, 1925)). The accumulating immigration statistics were making obvious another important change in immigration to the U.S. – a reduction in the proportion of low-skilled workers among the admitted immigrants (BLS, 1927).

This context provides the foundation for our analysis. The quota system did not deport existing immigrants or reduce labor supply overnight. Instead, it cut off future inflows, gradually slowing the growth of the workforce in high-immigration areas, as ordinary attrition from local labor markets due to retirements and worker out-migration was no longer being replaced at the same rate by new immigrants.[7] Because the severity of the quota-induced decline depended on each area's historical immigrant composition, the restrictions generated large and plausibly exogenous cross-market variation in the change in low-skilled labor supply (Abramitzky et al., 2023; Tabellini, 2020). This variation underlies the empirical strategy that follows, enabling us to examine how immigration restrictions—rather than expansions—affected wages.

3 Theoretical Framework

We develop a simple framework to guide our empirical analysis. Here we present and discuss the basic model results; a more detailed analysis is found in Online Appendix B. We focus on how a negative labor-supply shock that disproportionately reduces the supply of low-skilled workers affects wages in local labor markets, and how differences across industries in production technologies and labor mobility generate heterogeneous wage responses. We interpret the model as describing outcomes at the level of a regional labor market–industry pair.[8]

[7] This would be the likely effect on labor supply of most currently proposed policies to restrict immigration in the US and the EU—they would not remove existing workers from the labor force, but would substantially diminish the flow of new workers into the labor force. Although much press coverage of the Trump administration's immigration policy focuses on deportation, the number of workers actually being deported is small compared to the reduction in the number of migrants coming into the US.

[8] The framework is partial equilibrium: local markets take the rental rate of capital and product prices as given, and capital and labor responses are modeled at the industry–market-cell level. A full general-equilibrium model with endogenous national factor prices, product markets, and complete interregional migration would require additional assumptions that extend beyond the scope of our empirical analysis.

Economic Setting. Building on Borjas (2014), we consider production in regional labor market–industry cells indexed by (i, j). Each cell produces output Q_{ij} using capital K_{ij}, low-skilled labor $L_{1,ij}$, and skilled labor $L_{2,ij}$. Production follows a nested CES structure:[9]

$$Q_{ij} = \left[(1 - \alpha_{ij})K_{ij}^{\delta_{ij}} + \alpha_{ij} L_{ij}^{\delta_{ij}}\right]^{1/\delta_{ij}}, \qquad L_{ij} = \left[\theta_{1,ij} L_{1,ij}^{\beta_{ij}} + \theta_{2,ij} L_{2,ij}^{\beta_{ij}}\right]^{1/\beta_{ij}}. \tag{1}$$

The elasticities of substitution between capital and aggregate labor and between low-skilled and skilled labor in cell (i, j) are given by $\sigma_{KL,ij} = \frac{1}{1-\delta_{ij}}$ and $\sigma_{12,ij} = \frac{1}{1-\beta_{ij}}$, respectively. Factor prices are r for capital (determined in a national capital market and taken as given locally), $w_{1,ij}$ for low-skilled labor, and $w_{2,ij}$ for skilled labor. The parameters α_{ij}, $\theta_{1,ij}$, $\theta_{2,ij}$, and the substitution elasticities may differ across industries, capturing heterogeneity in capital intensity, production technologies, and the scope for substituting capital for labor.

We assume that in each (i, j) cell both labor groups contract, with a disproportionately larger fall in low-skilled labor:

$$\frac{dL_{1,ij}}{L_{1,ij}} < \frac{dL_{2,ij}}{L_{2,ij}} < 0. \tag{2}$$

This asymmetry—a larger reduction in low-skilled relative to skilled labor—reflects the historical shock and underlies the model's key predictions regarding differential wage responses across skill groups within each regional labor market–industry cell.

Short-Run Wage Effects. In the short run, the capital stock K_{ij} is fixed in each cell, so local output must be produced using fewer workers after the imposition of immigration restrictions. Log-linearizing the first-order conditions for the nested CES structure yields

$$d \ln w_{1,ij} - d \ln w_{2,ij} = \frac{1}{\sigma_{12,ij}} \left(d \ln L_{2,ij} - d \ln L_{1,ij}\right) > 0. \tag{3}$$

Equation 3 implies that the relative wage of low-skilled workers, $w_{1,ij}/w_{2,ij}$, increases whenever the decline in low-skilled labor is larger than that of skilled labor and $\sigma_{12,ij}$ is finite. The magnitude of this increase depends on the degree of substitution between skill groups: a lower $\sigma_{12,ij}$ (i.e., stronger complementarity) produces a larger divergence in relative wages.

In absolute terms, the wage of low-skilled workers $w_{1,ij}$ rises unambiguously in the short run. Because the total labor force in cell (i, j) contracts while capital K_{ij} is fixed, the marginal product of labor increases, and this effect is strongest for the group whose supply falls most sharply.[10] Regardless, the overall outcome is a narrowing of the local skill premium in more exposed markets.

Medium-Run Adjustments. Over the medium run, both capital and labor can respond to the wage changes generated by restrictions on immigration. The transition following the restrictions

[9] To ensure that the production function is well behaved and that the elasticities of substitution are finite and positive, we assume $\alpha_{ij}, \theta_{1,ij} \in (0, 1)$, $\theta_{1,ij} + \theta_{2,ij} = 1$, and $\delta_{ij}, \beta_{ij} \leq 1$.

[10] The wage response for skilled workers, in contrast, is theoretically ambiguous: it depends on the pattern of substitution across inputs.

is gradual because both capital accumulation and labor reallocation involve adjustment costs and frictions that differ across industries. This transitional phase is critical for interpreting the dynamic wage effects we estimate, as it captures the gradual process by which firms and workers respond to immigration restrictions across regional labor market–industry cells.

Capital Adjustment. Empirical evidence from early twentieth-century U.S. industries suggests gradual capital investment dynamics (Abramovitz and David, 1973; David, 1977; Goldin and Katz, 1998). We capture this by allowing capital in each cell (i, j) to adjust only partially toward its new long-run level. Let

$$d \ln K_{ij,MR} = \gamma_{ij}\, d \ln K_{ij,LR}, \qquad 0 \leq \gamma_{ij} \leq 1, \tag{4}$$

where $d \ln K_{ij,LR} = \ln K_{ij,LR} - \ln K_{ij,0}$, $K_{ij,LR}$ is the long-run equilibrium capital stock, $K_{ij,0}$ is the pre-shock level, and γ_{ij} measures the degree of capital adjustment in industry i and region j. A smaller γ_{ij} reflects slower investment dynamics and thus more persistent wage effects.[11]

In practice, the adjustment speed γ_{ij} is not uniform across industries. Expanding capital in response to higher wages can be costly in sectors that rely on large, specialized fixed capital with high replacement costs , implying a low γ_{ij} and thus more persistent wage increases after a negative labor supply shock. One would expect higher γ_{ij} values in sectors in which capital can be expanded more flexibly, for example, by adopting relatively inexpensive machinery or renting equipment.

Using the first-order conditions of the nested CES production function and substituting Equation 4, the medium-run own-wage elasticity of skill group $i \in \{1, 2\}$ with respect to its own labor supply in cell (i, j) is

$$\left.\frac{\partial \ln w_{k,ij}}{\partial \ln L_{k,ij}}\right|_m = \frac{1}{\sigma_{KL,i}\sigma_{12,i}} \left[\sigma_{12,i} \left((S_{K,ij}\gamma_{ij} + S_{L,ij} - 1) \frac{S_{k,ij}}{S_{L,ij}} \right) + \sigma_{KL,i} \left(\frac{S_{k,ij}}{S_{L,ij}} - 1 \right) \right], \quad k = 1, 2. \tag{5}$$

where $S_{K,ij}$ and $S_{L,ij}$ denote the cost shares of capital and total labor in (i, j), and $S_{k,ij}$ is the cost share of skill group k within total labor (so $S_{1,ij} + S_{2,ij} = S_{L,ij}$). When $\gamma_{ij} = 0$, the elasticity simplifies to its short-run value; as γ_{ij} increases, capital accumulation restores the capital–labor ratio and reduces the magnitude of the wage response.

Intuitively, in cells with higher capital shares and high adjustment costs (low γ_{ij}), firms are more constrained in expanding capital after the quotas, so wages respond strongly and remain elevated for longer. In cells with low capital shares or lower adjustment costs (high γ_{ij}), firms can more easily expand capital or adopt more capital-intensive methods, leading to smaller and less persistent wage effects.

It is interesting to consider the results of Clemens et al. (2018) in light of this aspect of the model. They find that the ending of the Bracero program in the early 1960s, which caused large reduction in the supply of farm labor in the southwest, had a very short-lived effect on agricultural

[11] When $\gamma_{ij} = 0$, the economy in cell (i, j) remains in the short run with fixed capital; when $\gamma_{ij} = 1$, capital in that cell has fully adjusted to its new steady-state level.

wages as farmers switched to more capital intensive methods for producing various crops, or to producing outputs that required less labor intensive technologies. As the authors note, these alternative production processes were well known in the agricultural sector. Also, in post-war American agriculture, the capital services necessary to a capital intensive agricultural production process were often obtainable in well-established rental markets. In the language of the model, the elasticity of substitution was high and the costs of adjusting capital low, in agriculture relative to other industries, allowing a relatively rapid adjustment of wages to their long run values following a negative labor supply shock.

Labor Mobility. Wage adjustment in the medium run also depends on the ability of workers to move across regional labor markets and industries. Immigration restrictions did not affect all (i, j) cells equally—areas and sectors that had previously relied heavily on foreign-born workers experienced larger local labor shortages and, consequently, larger wage increases. Over time, domestic migration and occupational mobility could help offset these imbalances, but the speed and extent of this adjustment depend on the degree of labor mobility. Formally, we represent local labor-supply responses in reduced form as

$$d \ln L_{k,ij}\big|_{MR} = \pi_{k,ij} + \rho_{k,ij}\, d \ln w_{k,ij}\big|_{MR}, \qquad k = 1, 2, \tag{6}$$

where $\pi_{k,ij} < 0$ denotes the initial, exogenous decline in labor supply for group k caused by immigration restrictions in cell (i, j), and $\rho_{k,ij}$ measures the local elasticity of labor supply with respect to wages. A higher $\rho_{k,ij}$ reflects greater labor mobility or stronger labor-force participation responses. Industries that draw on more mobile workers (e.g., construction or some agricultural activities) may have higher $\rho_{k,ij}$, while sectors tied to specific locations or skills (e.g., mining, heavy manufacturing) may have lower $\rho_{k,ij}$.

Substituting Equation 6 into the medium-run wage equations yields a dynamic system in which wages and labor quantities jointly adjust over time. Although the full solution is complex, the economic interpretation is straightforward. When labor mobility is limited (small $\rho_{k,ij}$), local labor markets adjust mainly through wages: labor shortages persist and wage effects remain elevated. When labor mobility is higher (large $\rho_{k,ij}$), migration flows and participation responses help equalize labor supply across regions and industries, compressing wage differentials more rapidly.

Currently, the literature offers conflicting evidence on the scope of internal migration in response to labor-supply shocks. Historical studies emphasize substantial frictions: Ferrie (1997) documents moderate geographic and occupational mobility in the early twentieth century, and Collins and Wanamaker (2015) show that migration between southern and northern labor markets was selective and incomplete, with only a small share of workers relocating despite large regional wage differentials. By contrast, recent research finds stronger internal migration responses to immigration or labor-market shocks in modern settings (Abramitzky et al., 2023; Price et al., 2020). For the case of low-skilled workers during our study period, the extent of such adjustment remains uncertain: mobility costs, occupational specialization, and limited information flows likely constrained large-scale movement. The degree of labor mobility therefore determines whether wages could equalize across space and at what speed.

Long-Run Adjustment. In the long run, both capital and labor in each cell (i, j) have had sufficient time to adjust to the post-restriction environment. Capital accumulation is complete and firms have fully restored their desired capital–labor ratios, while wage differences across regions and industries would be further attenuated by migration and reallocation. The long-run equilibrium reflects a new composition of the labor force: immigration quotas permanently reduced the supply of low-skilled workers relative to high-skilled workers, so the steady-state wage ratio depends on the new relative supply of each group.

4 Wages, Labor Markets, and Exposure Measures

We compile and harmonize annual wage data for low-skilled workers across multiple labor markets and industries in the United States between 1910 and 1929. We then construct measures of each labor market-industry pair's exposure to immigration restrictions based on the historical country-of-origin composition of its immigrant population, exploiting the fact that the quotas restricted immigration from some countries far more than others. Online Appendix A provides additional and more detailed information on the data sources, sample construction, and exposure measures.

4.1 Data Sources

Our analysis draws on several historical wage surveys covering low-skilled workers across a wide set of local labor markets and industries between 1910 and 1929. We compile these sources into a pooled dataset that reports hourly wages for narrowly defined laborer occupations, measured at the city- or state-industry level. The combined sample includes 2,973 wage observations for 399 labor market–industry pairs spanning 125 local labor markets and 18 industries. We also incorporate two supplementary cross-sections—municipal laborers wages from 1928 and wage data from the 1940 Census—to validate our main findings and assess longer-run wage patterns. Table 1 summarizes all datasets, their structure, and the number of observations and labor market–industry pairs.[12]

Manufacturing and Mining. The BLS industry surveys provide the most detailed evidence on low-skilled industrial wages in our period of interest. These surveys report mean wages at the state or city level for narrowly defined occupations in a broad set of manufacturing and mining industries for years between 1910 and 1929. We focus on the lowest-paid occupations reported within each industry (typically labeled "laborer").[13] The surveys yield a sample of over 1,200

[12] Figure A1 shows the annual distribution of labor market–industry wage observations between 1910 and 1929.

[13] For the small number of industries without an occupation titled "laborer" we use the average wage for a large occupation at the bottom of the industry wage scale. Each industry contributes one occupation except the coal industry, for which we include the wages of three low-skilled occupations: pick miners, laborers working inside the mine, and laborers working outside of the mine.

Table 1. Summary of Historical U.S. Labor Markets Wage Data

Data Source (1)	Years (2)	Coverage (3)	Observations (4)	Labor Market-Industry Pairs (5)	
1. Pooled Sample (Main Analysis)	1910–29	Unbalanced	2,973	399	(125 LMs, 18 Ind.)
1.A. Manufacturing & Mining (BLS)	1910–29	Unbalanced	1,234	248	(48 LMs, 16 Ind.)
1.B. Construction (American Contractor)	1920-28	Unbalanced	292	58	(58 LMs, 1 Ind.)
1.C. Construction - Union Scales (BLS)	1910-28	Unbalanced	487	45	(45 LMs, 1 Ind.)
1.D. Agriculture (USDA)	1910–29	Balanced	960	48	(48 LMs, 1 Ind.)
2. Long-Term Wages (Census)	1940	Cross-section	399	399	(125 LMs, 18 Ind.)
3. Municipal Laborers (BLS)	1928	Cross-section	2,295	2,295	(2,295 LMs, 1 Ind.)

Notes: Wages are reported as actual earnings, negotiated scales, or hourly equivalents. Details on the construction of the wage variables for each data source are described in the text. Long-term wages are earnings per hour reported in the 1940 Census. The pooled sample combines datasets A–D. Coverage indicates balanced, unbalanced, or cross-sectional data structure.

industry–jurisdiction–year wage observations covering 16 industries in 48 jurisdictions.[14]

American Contractor. Throughout the 1920s, the trade journal *American Contractor* surveyed construction contractors in many cities, asking for the wages paid in their city to workers in various construction trades, including "laborers" who generally have the lowest reported wages. We compile observations for more than 50 cities from monthly surveys conducted between 1920 and 1928. When multiple observations are available for a city-year pair, we use the highest reported wage.

Construction Unions. *American Contractor* did not publish wage surveys prior to 1920; thus, the surveys provide limited information on geographic wage differentials for construction laborers in the pre-quota period. For that reason, we use a second data source on construction wages. From 1910 to 1929, the BLS did annual surveys of labor union leaders in various cities, asking for "union scale of wages and hours of labor." Construction trade unions were among those surveyed, and union officials in many cities reported the negotiated union wage for "building laborers."[15] While not direct evidence of actual wages paid, they provide consistent information on local wage standards and trends.[16]

Agriculture. Beginning in the early 20th century, the U.S. Department of Agriculture collected data on the average wages of agricultural laborers in each state (USDA,1943; Haines et al., 2018). We use information on daily and monthly wages for all 48 states from 1910 to 1929, converting daily wages to an hourly basis by assuming a 10-hour workday for farm laborers.

[14] Table A2 lists and describes the industries and associated occupations in the sample.

[15] Table A3 lists the cities covered by each construction source and flags whether a city appears in the American Contractor survey, in the union scale data, or in both.

[16] These data are less reliable than the BLS industry surveys as indicators of actual wages paid for two reasons. First, the scales are established minimums, and it was not unusual for unionized workers to be paid more than this minimum. Second, not all construction workers in a surveyed city would be paid according to the scale, although the BLS only reported scales for cities in which over 50% of the workers in an occupation were being paid according to the union scale (BLS,1913).

Long-Run Wages and Labor Market Characteristics. We supplement our data using the 1900-1940 Population Censuses for details on employment, foreign-born share, and other demographics at the state- or county-industry level (Ruggles et al., 2024). We calculate mean hourly wages for low-skilled workers in these pairs in 1940, the first year wage data was collected by the Census, to assess the long-term wage impacts of the quotas. Additionally, we gather socioeconomic data for that period from the Historical, Demographic, Economic, and Social Data of the United States through ICPSR (Haines et al., 2010).

4.2 Exposure to Immigration Restrictions

Definition of Exposure Measure. A central empirical challenge is to map each wage observation onto the correct local labor market — the geographic area that supplied the low-skilled workers whose wages the observation reflects. We formalize this by defining the *Relevant Labor Market* (RLM) for a given wage observation w_{ijt} as the geographic area that supplies low-skilled labor to the establishments that underlie the mean wage for industry i in jurisdiction j at time t. In the period we analyze, the county is the most practical and economically meaningful unit for approximating an RLM: low-skilled workers typically commuted short distances (on foot or local transit) and industry employment was frequently clustered in a small set of counties within a state. Where the wages are at the city level, the RLM is simply the county in which that city is located; where wage reports are at the state–industry level, we include counties in state j as part of the relevant labor market with a probability weight based on the percentage of the state's total employment in industry i found in that county.[17]

Formally, we first compute a county-level exposure q_c that captures how much of county c's working-age (16-65) male labor force in 1910 was composed of immigrants from sending regions that later faced restrictive quotas:

$$q_c = \sum_r \frac{FB_{rc1910}}{LF_{c1910}} \times QI_r \tag{7}$$

where FB_{rc1910} is the number of working-age foreign-born males from sending region r in county c in the 1910 full count census, LF_{c1910} is the county's working-age male labor force in 1910, and QI_r is the quota intensity ratio for region r taken from Abramitzky et al. (2023). The quota intensity QI_r is a ratio defined as the difference between an estimate of the unrestricted flow from a region in the absence of quotas and the quota slots for that region, normalized by the unrestricted flow.[18] This ratio will be zero if the number of allocated slots for a region is greater than or equal to the estimate of the unrestricted flow from that region and one if the quota is set equal to zero. The

[17] Figure A2 provides justification for this choice: the figure plots the within-state distribution of the 1910 industry employment across counties for the state-industry pairs in our sample and shows that the geographic distribution of employment was highly uneven: a small number of counties typically accounted for a large share of total industry employment. In Online Appendix D, we show that our results are robust to a variety of alternative exposure measures.

[18] The unrestricted flow from a region is estimated using a model that predicts immigration in the 1920s based on historical immigration flows for several pre-1915 decades.

QI_r values are large for the “high restriction” sending regions (0.925 on average) and near zero for the “low restriction” or exempt regions (0.07 on average).[19]

Second, we combine the county exposure measure q_c with the 1910 county-industry employment shares α_{ijc} to construct an RLM-industry exposure measure by calculating the weighted average of the q_c values for the counties in that state:

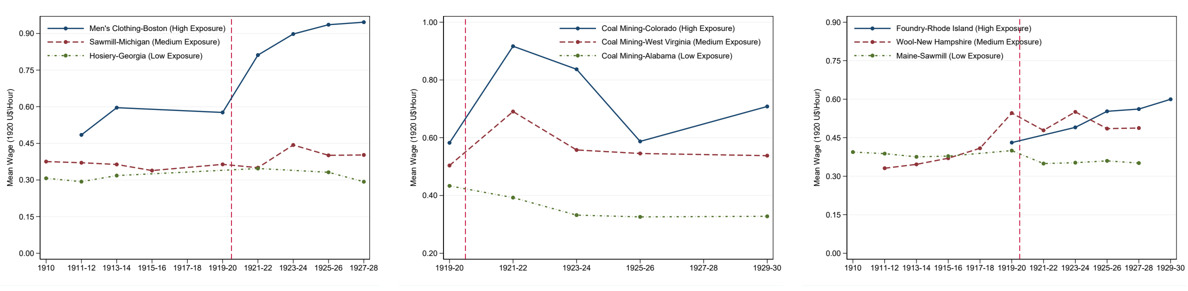
$$QE_{ij} = \sum_{c \in j} \alpha_{ijc} \times q_c \tag{8}$$

If the jurisdiction is a city, the quota exposure measure for the observation is simply the quota exposure measure for the county in which the city is located, that is, $QE_{ij} = q_c$.[20] We adapt the RLM industry approach to our state-level agricultural data by weighting county-level exposure by the county’s share of state farm labor employment and aggregating across all counties in the state.

Descriptive Evidence. Our wage data show a clear and consistent pattern: local labor markets more dependent on immigrants from highly restricted regions experienced stronger wage increases after the quotas took effect. Figure 2 illustrates this for the manufacturing and mining sample. Panel (a) plots mean hourly wage trajectories (in 1920 dollars) for three representative pairs: men’s clothing in Boston (high exposure), sawmills in Michigan (medium exposure), and hosiery in Georgia (low exposure). Wages follow similar trends before 1920 but diverge sharply afterward, with the highly exposed industry showing a pronounced and sustained increase, the medium-exposure industry a moderate rise, and the low-exposure industry little change. Panels (b) and (c) further demonstrate how our data capture both cross-industry and within-industry variation by showing that similar patterns appear within a single industry (coal mining) or region (New England).

Figure 2. Manufacturing & Mining Wage Dynamics Across Exposure Levels

(a) All RLM-Industry Pairs | (b) Same Industry - Coal Mining | (c) Same Region - New England



Notes: High, medium, and low exposure levels correspond to quota exposure values in the top, middle, and bottom terciles, respectively. Mean hourly wages are expressed in 1920 dollar terms.

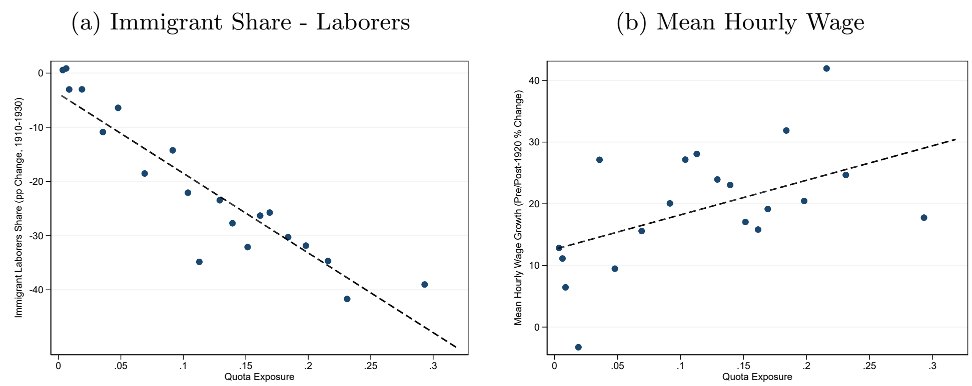
Figure 3 generalizes the patterns from Figure 2 to the full set of RLM–industry observations.

[19] See Table A1 and Abramitzky et al. (2023) for details on the model used to estimate unrestricted flows.

[20] The weights α_{ijc} are constructed from the (pre-restrictions) 1910 full-count census industry employment shares to avoid mechanical endogeneity from post-1910 employment shifts. In Online Appendix D, we show that our results are robust to using 1900 instead of 1910 as the base year for calculating q_c and α_{ijc}.

It presents binned scatter plots, estimated after absorbing industry fixed effects, that summarize the relationship between quota exposure, immigrant employment, and wages. Panel (a) shows a strong negative relationship between exposure and the change in the immigrant employment share among laborers from 1910 to 1930, while panel (b) shows a positive relationship between exposure and real wage growth.

Figure 3. Changes in Immigrant Share and Hourly Wages by Quota Exposure



Notes: Panel (a) plots the percentage point change in the share of immigrant laborers between 1910 and 1930 against RLM–industry quota exposure. Immigrants are defined as foreign-born individuals who arrived in the U.S. within the past ten years from quota-restricted countries. Panel (b) shows the percent change in mean hourly wages (in 1920 dollars) before and after 1920 by quota exposure. Both panels are based on bin-scatter plots with 20 bins, absorbing industry fixed effects.

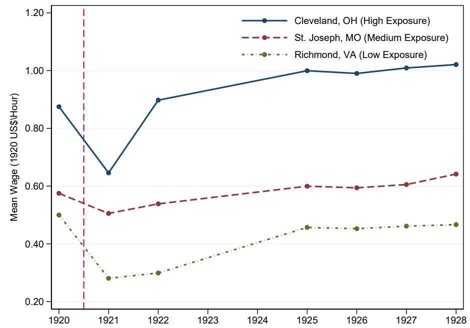
Figures 4 and 5 show similar patterns in sectors outside manufacturing and mining. In construction, whether using reported hourly wages (panels a and c of Figure 4) or union scales (panels b and d of Figure 4), higher exposed cities experience larger post-quota wage gains and steeper declines in immigrant employment than medium- and low-exposure cities.

Because the American Contractor source includes only one pre-quota observation (1920), panels (a) and (b) highlight post-quota differences in wage levels: in panel (a), wages in Cleveland (high exposure) rise sharply through the 1920s, those in St. Joseph (medium-exposure) increase modestly, and those in Richmond (low-exposure) remain roughly flat. Panel (c) shows that, on average, real wages fell overall relative to 1920 but declined less in cities with higher exposure. The wage scales panel (b) plot for Pittsburgh (high exposure), Los Angeles (medium exposure), and Des Moines (Low exposure) shows a sharp reversal after the war, with gains persisting through the 1920s.

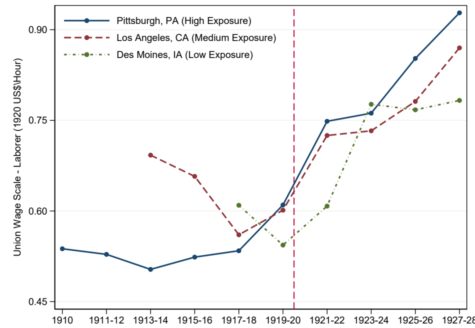
In agriculture (Figure 5), states in the top exposure tercile exhibit both the sharpest drop in immigrant farm labor and the strongest wage growth during the 1920s. Panel (a) highlights three representative states—California (high exposure), Wisconsin (medium exposure), and Oklahoma (low exposure)—illustrating that wages were relatively stable before 1920 but diverged in the 1920s: California experienced the strongest and most sustained wage growth, Wisconsin rose moderately, and Oklahoma remained largely flat. Panel (b) generalizes this pattern across all states, showing that the decline in the immigrant share of farm labor and wage growth followed a clear gradient by exposure tercile.

Figure 4. Construction Sector Wage Trends and Changes by Exposure Level

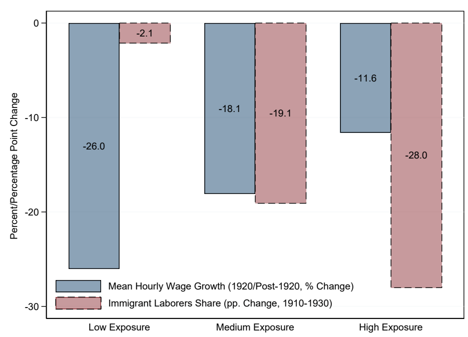
(a) American Contractor - Wage Trends



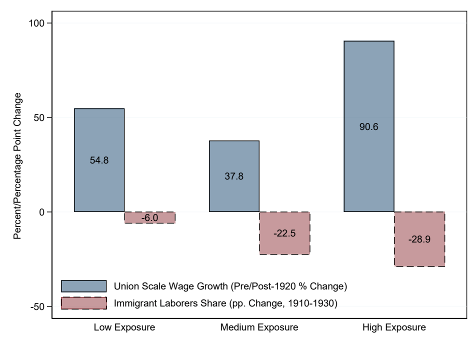
(b) Construction Union - Wage Trends



(c) American Contractor - Pre/Post 1920



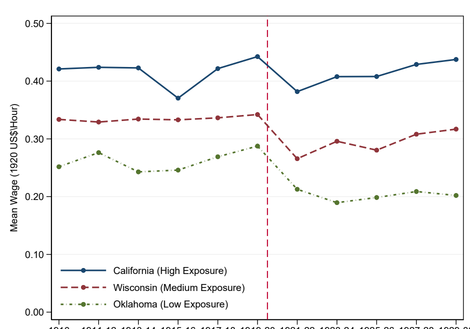
(d) Construction Union - Pre/Post 1920



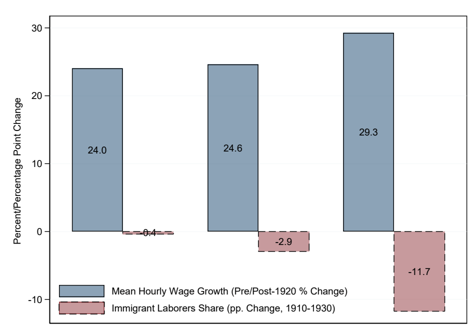
Notes: High, medium, and low exposure levels correspond to quota exposure values in the top, middle, and bottom terciles, respectively. Mean hourly wages are expressed in 1920 dollars. Panels c-d show the relationship between quota exposure and two outcomes–mean hourly wage growth (percent change comparing pre-1920 to post-1920 average) and the change in immigrant laborers' share (percentage point change from 1910 to 1930)– grouped by exposure tercile. Immigrant laborers are defined as foreign-born individuals who arrived in the United States within the past ten years from quota-restricted countries.

Figure 5. Agriculture Sector Wage Trends and Changes by Exposure Level

(a) Farm Laborers - Wage Trends



(b) Pre/Post 1920



Notes: High, medium, and low exposure levels correspond to quota exposure values in the top, middle, and bottom terciles. Mean hourly wages are expressed in 1920 dollars. Panel (b) shows the relationship between quota exposure and two outcomes: mean hourly wage growth (percent change comparing pre-1920 to post-1920 average) and the change in immigrant laborers' share (percentage point change from 1910 to 1930), where states are grouped by exposure terciles. Immigrant laborers are defined as foreign-born individuals who arrived in the United States within the past ten years from quota-restricted countries.

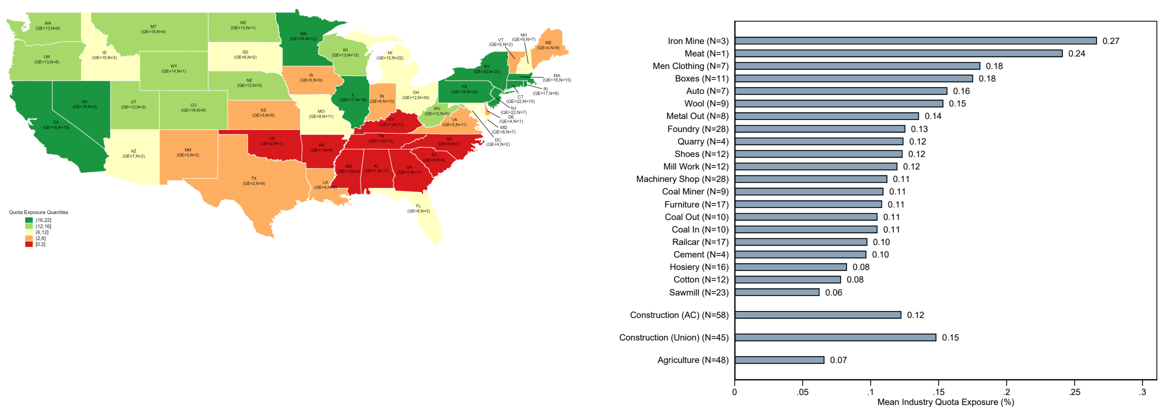
4.3 Pooled Sample

We pool RLM-industry pairs from the BLS Manufacturing & Mining, American Contractor Survey, BLS Survey of Construction Unions, and USDA survey on farm labor wages into one dataset to generalize our analysis.[21] Figure 6 provides a geographic and industrial overview of the pooled data. Panel (a) maps the mean exposure across states, indicating the mean exposure measure for RLM-industry pairs and the number of RLM-industry pairs in our analysis and shading each according to its quartile of average RLM–industry exposure. High-exposure RLM-industry pairs cluster in the Midwest and Northeast.[22] Panel (b) shows mean exposure by industry, highlighting substantial heterogeneity: industries such as men's clothing and machine shops exhibit the highest exposure levels, whereas agriculture and construction are generally less affected.[23]

Figure 6. Geographic and Industrial Distribution of Observations and Exposure

(a) Geography

(b) Industry



Notes: The map in panel (a) plots the quartiles of mean exposure across the RLM by industry pairs within a state. Each state has a label indicating its mean RLM-industry exposure measure QE and the number of RLM-industry pairs in the state N. The chart in panel (b) shows the mean RLM-industry exposure measure by industry, and each label lists the number of RLM-industry pairs observed in each industry.

Figure 7 summarizes the main empirical patterns and outlines our research design. Panels (a) and (b) document how immigration flows evolved before and after the restrictions. Panel (a) plots the change in the foreign-born share using decennial census data, while Panel (b) uses reported year of arrival to construct a proxy for annual immigration flows. Both panels show a pronounced peak in 1910 and 1914, respectively, a sharp wartime decline during the 1910s, and a further, sustained drop during the 1920s following the introduction of the quotas. The decline is substantially larger

[21] Throughout the analysis, we control for industry-by-year fixed effects to account for differences in the samples and data collection procedures. We also present results for each dataset separately.

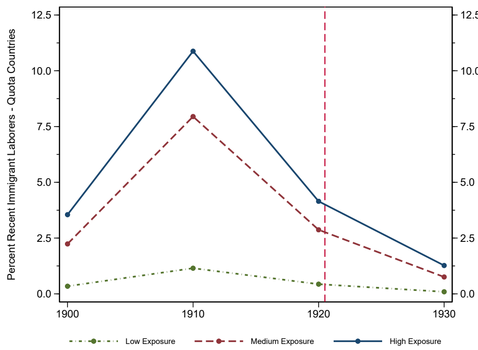
[22] The observations are distributed very similarly to the overall population in 1910, with the Midwest region having the highest number of RLM-industry pairs with 129 (36%), followed by the Northeast and the South with 95 and 93 (26%), respectively, and the West with 38 overall (12%).

[23] As shown in Table A4, the pooled dataset exhibits substantial variation in exposure to the 1920s immigration quotas across RLM–industry pairs. Markets in the highest exposure group had a much larger foreign-born population in 1910 and a distinctly different composition by country of origin. The share of immigrants from high-restriction countries rises from roughly 28 percent in the lowest exposure group to 43 percent in the highest, underscoring that the exposure measure captures meaningful variation in dependence on restricted source regions.

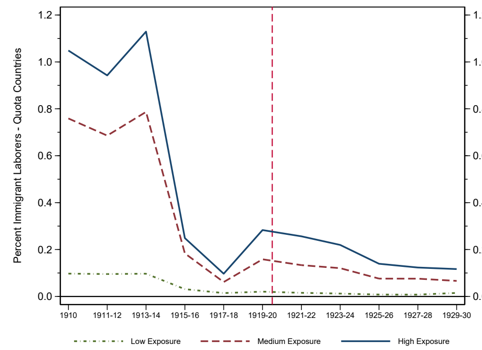
in local labor markets with higher pre-quota exposure, consistent with the design of our exposure measure. The year-of-arrival proxy also shows a brief rebound in immigration after World War I that quickly reversed once the restrictions took effect.

Figure 7. Immigration and Wage Trends by Exposure Level - Pooled Sample

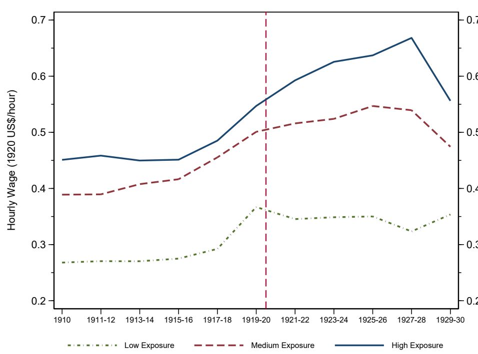
(a) Immigrant Laborers Share - Decennial Census



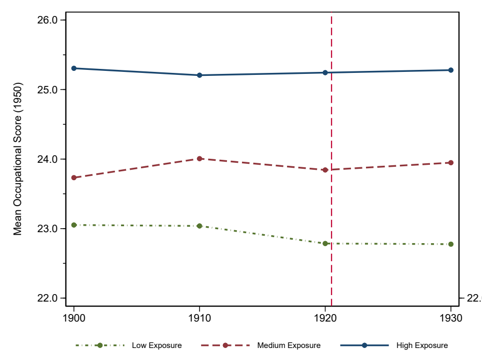
(b) Immigrant Laborers Share - Year of Arrival



(c) Mean Hourly Wage (Laborers)



(d) Mean Occupational Score (RLM-Industry)



Notes: Panels (a) and (b) show how the share of recently arrived immigrant laborers changed over time across RLM-industry pairs grouped into low-, medium-, and high-exposure terciles. Panel (a) uses decennial census tabulations and panel (b) uses year-of-arrival information to proxy annual inflows. Panels (c) and (d) compare wage responses across the same terciles. Panel (c) plots mean real hourly wages (in 1920 dollars) for low-skilled laborers based on the data collected in the analysis. Panel (d) displays mean occupational income scores averaged across all workers in each RLM-industry pair.

Panels (c) and (d) focus on wage and income dynamics across exposure groups. Panel (c) plots mean hourly wages for laborers by exposure terciles and shows that wages evolved relatively in a similar manner across groups before 1920 but rose more rapidly afterward in highly exposed RLM–industry pairs.[24] Panel (d) reports mean occupational income scores averaged across all workers in each industry—a commonly used proxy for earnings in historical research. Unlike the wage data, these scores show almost no change across or within exposure terciles, underscoring that wage proxies based on occupational income scores likely understate the true wage effects of the quotas. Together, the panels suggest that immigration restrictions led to sharper declines in immigrant inflows and stronger wage growth in high-exposure markets.

[24]The drop in mean wages in 1929 for the high- and medium-exposure group represents partially a composition change in the industries reporting wages in different years.

5 Empirical Strategy

Having established descriptive patterns in the raw data, we now turn to a more rigorous econometric analysis to examine whether the observed relationships can be given a causal interpretation. In particular, we test whether local labor markets that were more exposed to the 1920s immigration restrictions experienced larger losses of immigrant workers and correspondingly higher wage growth for low-skilled occupations.

Our analysis proceeds in two steps. We first verify that relevant labor markets with higher exposure to the quotas experienced larger declines in immigrant and subsequently overall employment during the 1920s. We then assess whether these declines translated into stronger wage growth among low-skilled workers. To do so, we estimate versions of the following baseline specification:

$$y_{ijt} = \delta_{ij} + \gamma_{it} + \beta(QE_{ij} \times Post_t) + \varepsilon_{ijt} \tag{9}$$

where y_{ijt} denotes the outcome of interest—log mean hourly wage, immigrant employment share, or log total employment—for RLM–industry pair ij in year t. The variable of interest is the interaction between the quota exposure measure QE_{ij} and the post-1920 indicator $Post_t$, which captures the period after immigration restrictions were implemented.[25] The coefficient β is identified by comparing relevant labor markets with different pre-quota shares of workers from restricted regions before and after 1920. Labor-market-by-industry fixed effects (δ_{ij}) absorb permanent differences in wage levels and immigrant composition across markets, while industry-by-year fixed effects (γ_{it}) account for time-varying industry-wide shocks.[26]

Along with our main estimation Equation 9, we explore the dynamic evolution of wage effects by estimating an event-study specification:

$$ln(w_{ijt}) = \delta_{ij} + \gamma_{it} + \sum_{k \neq 1920} \beta_k(QE_{ij} \times I_k) + \varepsilon_{ijt} \tag{10}$$

where $ln(w_{ijt})$ is the log mean hourly wage of RLM-industry pair ij in year t, and I_k is a year indicator equal to one in year k and zero otherwise, using 1920-the year before the quota laws-as the reference year. This specification allows us to test for pre-trends and trace the timing of the post-1920 adjustments.[27]

Because our treatment variable is continuous, identification relies on a "strong parallel trends" assumption in the sense of Callaway et al. (2024). We assume that, in the absence of immigra-

[25] There is some ambiguity about when the "post" period should begin. As discussed above, the war significantly reduced immigration starting in 1915. We follow the existing literature and choose 1920 as the beginning of the "post" period. Our event study specifications check our decision regarding the beginning of the post-period. Moreover, we also test directly for the impact of the war on wages in Online Appendix D.

[26] In Online Appendix D, we validate that our analysis is robust to alternative definition of exposure measures, the inclusion of relevant covariates, including the initial foreign-born share, aggregation of the observations from RLM-industry level to RLM-level, exposure to WWI, and selection into the sample.

[27] We use year-pair dummies rather than individual year dummies to increase the precision of the estimates since our sample is an unbalanced panel with different industries appearing in different years. Combining consecutive years alleviates this measurement issue to some extent.

tion restrictions, outcomes in markets with differing exposure levels would have followed parallel trends. This assumption permits comparing changes across the exposure distribution and rules out systematic pre-policy divergence.[28] We provide several pieces of evidence to support this assumption, including an event-study analysis that directly tests for pre-trends in wages, and a series of robustness checks—including alternative exposure definitions, aggregation levels, and controls for WWI-era disruptions—reported in Online Appendix D.

6 Results

6.1 Immigration and Employment

Table 2. Exposure to Border Closure Policy and Foreign-Born Employment Shares

Sample:	**Pooled**	**Manufacturing & Mining**	**American Contractor**	**Construction Union**	**Agriculture**
Outcome: Employment Share	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
1. *All Foreign-Born*					
Quota Exposure X Post 1920	-0.300***	-0.403***	-0.177***	-0.123**	-0.168
	(0.0322)	(0.0411)	(0.0523)	(0.0564)	(0.115)
Pre-1920 Dependent Mean	0.28	0.32	0.22	0.29	0.15
2. *Quota Countries*					
Quota Exposure X Post 1920	-0.345***	-0.476***	-0.170***	-0.143***	-0.184*
	(0.0301)	(0.0375)	(0.0469)	(0.0365)	(0.0981)
Pre-1920 Dependent Mean	0.24	0.28	0.19	0.26	0.13
3. *Recent Immigrants - Quota Countries*					
Quota Exposure X Post 1920	-0.511***	-0.667***	-0.308***	-0.259***	-0.351***
	(0.0334)	(0.0404)	(0.0465)	(0.0546)	(0.0383)
Pre-1920 Dependent Mean	0.09	0.11	0.06	0.08	0.03
Quota Exposure - Mean	0.11	0.11	0.11	0.13	0.07
Quota Exposure - SD	0.08	0.08	0.10	0.09	0.06
Number of RLM-Industry Pairs	399	248	58	45	48
Observations (RLM-Industry Pair)	1,197	744	174	135	144

Notes: The table shows estimates of the interaction coefficient between the quota exposure measure QE and the post-policy change indicator. Column 1 includes results for the pooled sample, while columns 2-5 provide results by data source. Panel A uses the overall foreign-born employment share in the Relevant Labor Market (RLM) by industry pair as the outcome variable, while Panel B focuses on workers from quota-restricted countries, and Panel C looks at workers from quota-restricted countries that have been in the US for 10 years or less. All specifications incorporate industry-year and RLM-industry fixed effects. Each RLM-Industry pair has data from the 1910, 1920, and 1930 censuses, with robust standard errors clustered at the RLM by industry level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We begin by documenting how the immigration restrictions reshaped the composition of local labor markets. Using the 1910, 1920, and 1930 censuses, Table 2 shows that RLM–industry pairs more exposed to the quotas experienced substantially larger declines in immigrant employment. In the pooled sample (column 1), the interaction between quota exposure and the post-1920 period implies

[28] The "standard" parallel trends assumption would require the evolution of the foreign-born share and wages of RLM-industry pairs with different proportions of immigrants from restricted regions to evolve similarly regardless of their exposure level.

that an average-exposure market (0.11) saw a roughly 3.3 percentage-point reduction in its overall foreign-born share. The effect is even larger when focusing on immigrants from quota-restricted regions (-3.4) and peaks for recent arrivals from those regions (-5.1). These patterns confirm that the quotas sharply curtailed immigration and disproportionately affected local labor markets that had relied most heavily on restricted-origin workers.

The magnitude of these effects varies across datasets. The manufacturing and mining sample estimates in column 2 show the sharpest declines, while the construction datasets display smaller but still meaningful effects (columns 3-4). Agriculture estimates are smallest, in line with the relatively lower exposure of this sector to immigration (column 5). Despite differences in scope, all datasets show significant exposure-related declines in immigrant employment, confirming that the quotas induced broad, sector-wide contractions in immigrant labor supply.

Focusing on low-skilled labor reveals even more pronounced effects. Table 3 shows that the average-exposure RLM–industry pair experienced nearly a 10 percentage-point drop in the share of recent immigrants among laborers, far larger than the decline observed for the overall workforce.[29]

Table 3. Exposure to Border Closure Policy and Laborers Employment

Sample:	**Pooled** (1)	**Manufacturing & Mining** (2)	**American Contractor** (3)	**Construction Union** (4)	**Agriculture** (5)
1. *Recent Immigrants Share*					
Quota Exposure X Post 1920	-0.942***	-1.116***	-0.774***	-0.674***	-0.547***
	(0.0528)	(0.0597)	(0.115)	(0.149)	(0.0476)
Pre-1920 Dependent Mean	0.15	0.17	0.14	0.16	0.05
2. *Log Laborers Employment*					
Quota Exposure X Post 1920	-1.047***	-1.294***	-1.356*	-0.0712	0.360
	(0.279)	(0.360)	(0.734)	(0.488)	(0.449)
Pre-1920 Dependent Mean (Levels)	1,732	1,409	2,210	3,148	1,490
Quota Exposure - Mean	0.11	0.11	0.11	0.13	0.07
Quota Exposure - SD	0.08	0.08	0.10	0.09	0.06
Number of RLM-Industry Pairs	399	248	58	45	48
Observations (RLM-Industry Pair)	1,197	744	174	135	144

Notes: This table reports coefficients on the interaction between the quota exposure measure and a post-policy indicator. Row (1) uses the share (among laborers) of recent immigrants (arrived within ten years) from quota-restricted origins as the dependent variable. Row (2) reports results for the log number of laborers employed in each RLM–industry pair. Column 1 shows results for the pooled sample, while columns 2-5 provide results by data source. All outcomes are measured at the Relevant Labor Market (RLM) by industry level. All specifications include RLM-by-industry and industry-by-year fixed effects. Each RLM-industry pair appears in the 1910, 1920, and 1930 censuses. Robust standard errors, clustered at the RLM-by-industry level, are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Employment adjustments also varied across industries and demographic groups. In the pooled sample, the coefficient on log laborer employment is -1.05, implying that an RLM–industry pair with average exposure experienced roughly a 10–11 percent decline in total laborer employment after 1920. The contraction was largest in manufacturing and mining (-1.294), while agriculture

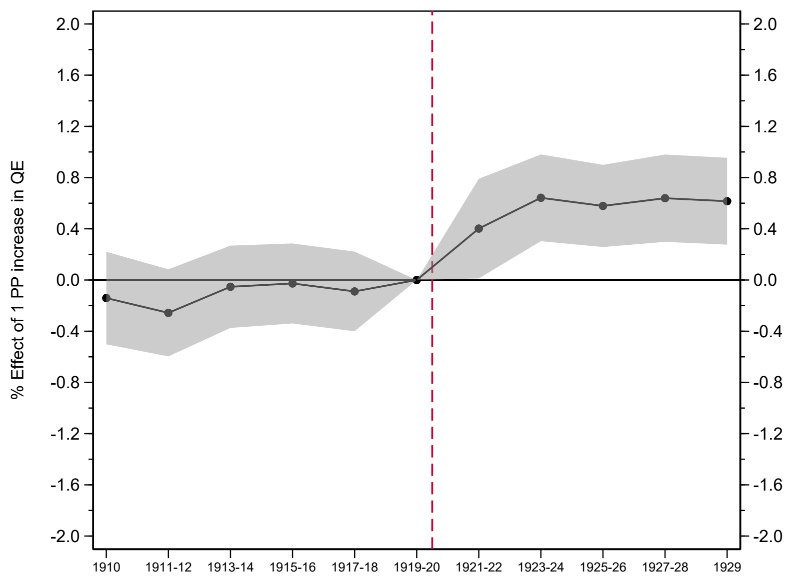
[29] Table C1 shows similar patterns for the broader occupational structure: reductions in immigration and employment are concentrated in low-skill occupations, while medium- and high-skill employment remains largely stable.

shows a small, statistically insignificant positive effect.[30] Overall, total employment among laborers fell more in high-exposure markets, but the ability to substitute away from restricted immigrants varied by industry and local labor market conditions.

6.2 Wages

Figure 8 presents event-study results for the pooled sample, providing a dynamic view of how wages evolved in markets with differing exposure to immigration restrictions. All pre-1920 coefficients are statistically insignificant and nearly flat, supporting the strong parallel trends assumption. Post-1920, coefficients rise sharply—peaking around 0.6 by the mid-1920s—and remain elevated throughout the decade. This implies that wages in highly exposed RLM–industry pairs (top tercile) grew roughly 12 percent more than in less exposed markets (bottom tercile).

Figure 8. Event-Study Estimates of Exposure to Border Closure Policy on Log Mean Hourly Wage



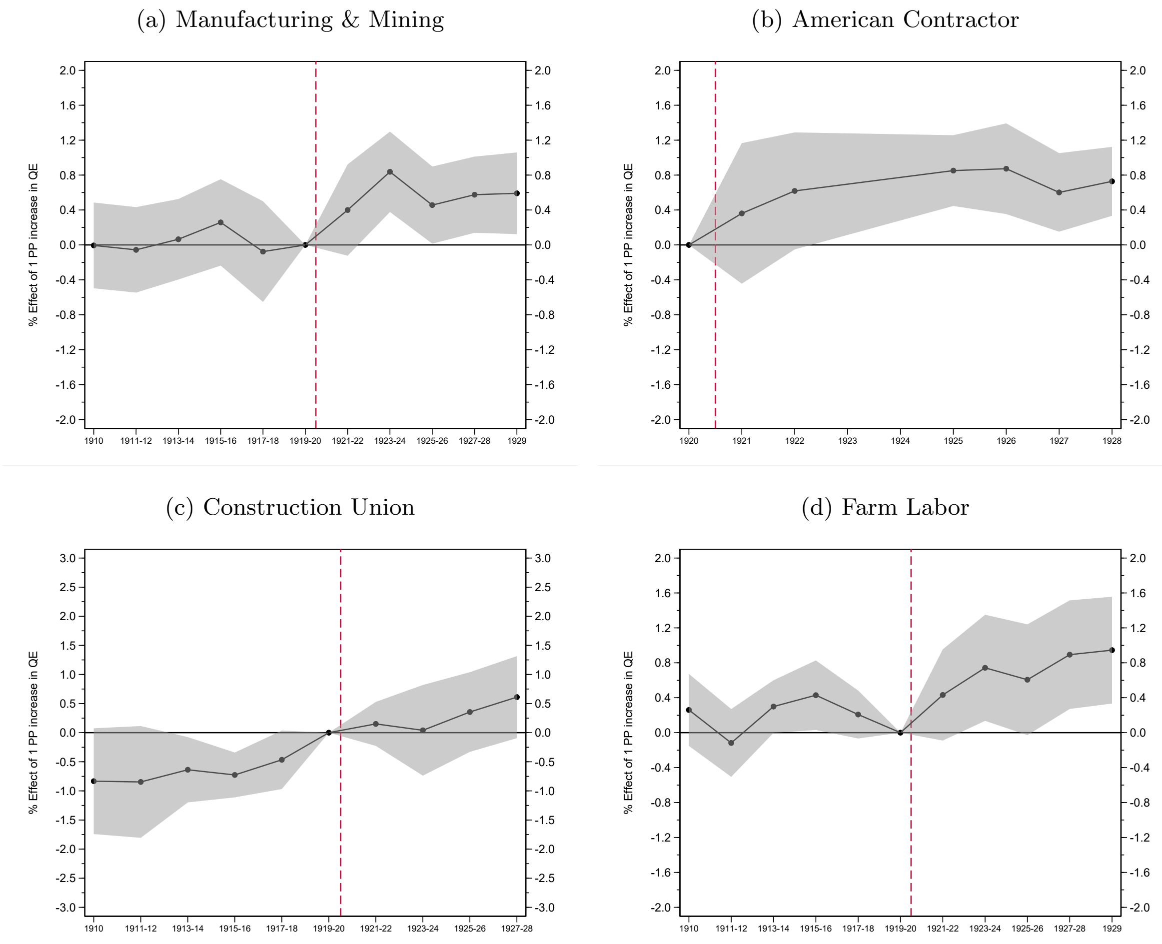
Notes: The figure plots the event-study coefficients of the interaction between the quota exposure measure and year indicator from Equation 10. All specifications include industry-year and RLM-industry fixed effects. Each RLM-industry pair has a varying number of observations from 1910-1929. The gray area shows 95% confidence intervals. Standard errors are clustered at the RLM-industry level.

Figure 9 shows the event study estimates of the effect of immigration restriction on log mean hourly wages for each dataset included in the pooled sample. The event study estimates are noisier and less precise due to the smaller sample sizes and the unbalanced structure of a large portion of the wage data. Nevertheless, all of the post-1920 estimates in the event studies are positive, and the majority are statistically significant, consistent with our pooled sample estimates.

[30] Table C2 provides further evidence on the composition of these adjustments. Native workers—especially non-white (mostly Black) workers—and immigrants from non-restricted regions increased their employment shares, but these gains did not fully offset the sharp contraction in restricted-origin immigrants. Consequently, total employment among laborers fell more steeply in high-exposure markets, even where partial replacement occurred.

In the mining and manufacturing sample (Panel a) , the pre-1920 coefficients remain flat around zero before 1920, while all but the 1921-22 coefficients are positive and significant. Panel (b) shows the estimates for construction workers in the American Contractor sample. Although we cannot test for the existence of pre-trends using this dataset, we can confirm that wages of construction laborers increased faster in cities with higher exposure, an estimated effect that became statistically significant in the second half of the decade.

Figure 9. Dataset-Specific Event-Study Estimates



Notes: The figures plot the event-study coefficients of the interaction between the quota exposure measure and year indicators from Equation 10 for each of the four datasets used in our analysis. All specifications include industry-year and RLM-industry fixed effects. Each RLM-industry pair has a varying number of observations from 1910-1929. The gray area shows 95% confidence intervals. Standard errors are clustered at the RLM by industry level.

In the Union Wage Scale sample (Panel c), the pre-1920 coefficients are negative and trend upward, with the 1913–14 and 1915–16 estimates statistically significant. This pattern indicates that strict parallel trends may not hold for this dataset and is consistent with Medici (2023), who documents that immigration helped expand organized labor—especially in urban construction trades—during these years. Following 1920, the coefficients turn positive and continue to rise, though none are statistically different from zero. This muted post-1920 wage response is in line with the near-zero employment effects for this sample shown in Table 3.

In the agricultural sample (Panel d), pre-1920 coefficients hover around zero and are uniformly insignificant, indicating no systematic pre-trend in wages by exposure. After 1920, coefficients turn positive and steadily rise, with effects strengthening and becoming statistically significant in the later 1920s. These patterns suggest that immigration restrictions ultimately boosted farm labor wages, aligning with Abramitzky et al. (2023), who show that more exposed rural areas mechanized and shifted toward less labor-intensive crops.[31]

Table 4 presents the pooled DID coefficients. In the pooled sample (column 1), our estimate implies that the average RLM-industry pair in our sample experienced a 6.2 percent increase ($= 0.615 \times 0.1$ where the outcome variable is mean log hourly wages) in mean hourly wages post-1920 relative to a pre-policy mean hourly wage of 26 cents. Each additional standard deviation increase in exposure (0.08) implied an additional 5 percent increase in mean hourly wages.

Table 4. Exposure to Border Closure Policy and the Wages of Low-Skilled Workers

Sample:	**Pooled** (1)	**Manufacturing & Mining** (2)	**American Contractor** (3)	**Construction Union** (4)	**Agriculture** (5)
Quota Exposure X Post 1920	0.615***	0.541***	0.665***	0.767***	0.527
	(0.148)	(0.147)	(0.205)	(0.275)	(0.339)
Pre-1920 Mean Hourly Wage ($)	0.26	0.24	0.61	0.40	0.19
Quota Exposure - Mean	0.10	0.11	0.13	0.15	0.07
Quota Exposure - SD	0.08	0.08	0.10	0.10	0.06
Number of RLM-Industry Pairs	399	248	58	45	48
Observations (RLM-Industry Pairs)	2,973	1,234	292	487	960

Notes: The table presents estimates of the interaction coefficient between quota exposure measure QE and the post-policy change indicator. The outcome variable is the RLM-industry log mean hourly wage for low-skilled workers. Column 1 presents results for the pooled sample, and columns 2-5 present results for each data source separately. The Post variable is an indicator for post-1920. All specifications include RLM-industry and industry-year fixed effects. Each RLM-industry pair has a varying number of observations from 1910-1929. Robust standard errors, clustered at the RLM by industry, level in parenthesis.. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

Columns (2)–(5) of Table 4 present separate difference-in-differences estimates by dataset. The manufacturing and mining (Column 2) and American Contractor (Column 3) samples yield wage effects similar in magnitude to the pooled estimate, while the construction union sample (Column 4) produces a much larger coefficient of 0.767.[32] In contrast, the agricultural sample (Column 5) yields a positive but smaller and statistically insignificant estimate (0.527), consistent with Table 3, which shows a modest and imprecise increase in farm labor employment, and with the gradual, noisy post-1920 wage patterns in Figure 9.

Taken together, the pooled results from Table 2 and Table 4 imply that a 3 percentage point decline in the foreign-born share and a 9 percentage point decline in the share of recent immigrants

[31]Consistent with this mechanism, Section 8 documents that states with higher agricultural exposure increased their agricultural capital intensity after 1920 relative to less exposed states.

[32]The positive estimate is driven by the upward trend visible in Panel c of Figure 9. Because the union wage scales are benchmarks rather than actual wages—and only a subset of construction workers were unionized—this estimate should be interpreted cautiously.

were accompanied by a 6-8 percent rise in mean hourly wages. This translates into wage elasticities that are comparable in magnitude to estimates in both the historical and modern literature. For example, Goldin (1994) finds that a 1 percentage point increase in the foreign-born share reduced urban laborers' wages by roughly 1.5–3 percent, and Hatton and Williamson (1995) reports similar estimates. In more recent work, Borjas (2003) obtains elasticities from about –0.9 to –2.1, while Monras (2020) finds short-run responses between –0.7 and –1.4. Our findings fall squarely within these ranges, though they reflect wage movements during declining rather than rising immigration, and would not be expected to be strictly symmetric.

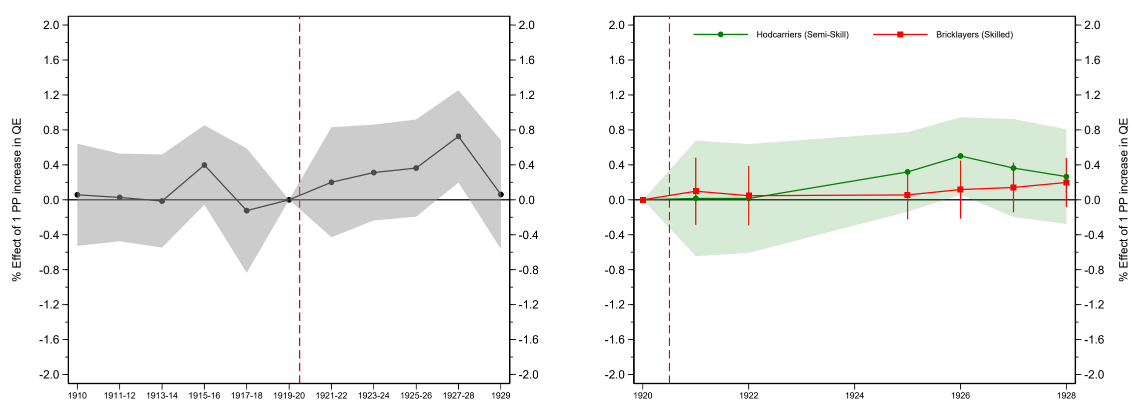
6.3 Additional Results

6.3.1 Semi-Skilled Workers

As part of our data collection process, we also digitized the wages of semi-skilled operatives using the BLS survey data on manufacturing and mining industries. For 12 of the 21 industries, we identified and recorded the average wage of a large occupation that was classed as a "semi-skilled operative" occupation, yielding a sample of 165 RLM-industry pairs.[33] Because the quotas disproportionately affected low-skilled labor, our model predicts more muted wage effects for semi-skilled workers.

Figure 10. Exposure to Border Closure Policy and the Wages of Semi-Skilled Workers

(a) Manufacturing & Mining (b) American Contractor



Notes: The figures plot the event-study coefficients of the interaction between our exposure measure and year indicators from Equation 10 for the log mean hourly wages of semi-skilled operatives in the manufacturing and mining sample in panel (a) and for hod-carriers (semi-skilled) and bricklayers (skilled) from the American Contractor dataset in panel (b). All specifications include industry-year and RLM-industry fixed effects. The gray area in panel (a), the green area in panel (b), and the red spikes in panel (b) show 95% confidence intervals. Standard errors are clustered at the RLM-industry level.

Panel (a) of Figure 10 presents the event-study estimates for semi-skilled operatives in manufacturing and mining. Both the pre- and post-1920 coefficients are noisy, statistically insignificant, and show no systematic pattern related to quota exposure. We also digitized data from the American Contractor surveys on the wages of hod-carries and bricklayers in different cities, considered semi-skilled and high-skilled occupations in the construction industry, respectively. Panel (b) displays

[33]Column 5 of Table A2 indicates industries for which we collected mean wages of semi-skilled workers.

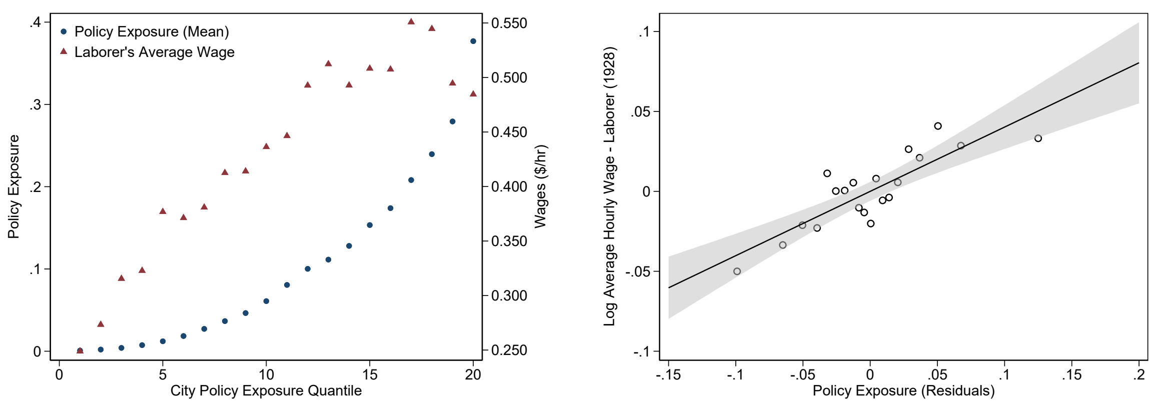
their event-study estimates: the coefficients remain small and insignificant throughout. Taken together, the muted effects we observe align with our theoretical predictions and contemporary expectations that the quotas would bind most strongly in low-skill labor markets.[34]

6.3.2 Municipal Street Laborers

In 1928, the BLS surveyed officials of around 2,600 municipalities regarding the wage rates of the low-skilled workers hired to clean and repair streets. The survey specifically asked for the wage paid to entry-level workers with no skill or experience. (BLS, 1929).[35] Our exposure variable for these wage data is q_c for the county in which the city is located. Towns and villages that overlapped or were very close to a county line in 1930 were excluded from the sample, leaving 2,295 cities and towns with usable wage data.[36]

Figure 11. Exposure to Border Closure Policy and Municipal Street Laborers' Wages

(a) Raw Data (b) Residualized Data



Notes: Panel (a) plots the relationship between city policy exposure quantile and policy exposure mean (blue circles) and municipal street laborers' mean hourly wage in 1928 (red triangles) for each quantile. Panel (b) plots the relationship between policy exposure and log mean hourly wage in 1928 for municipal street laborers using a binned scatter plot with 20 binds. Each city or town's policy exposure and wages are adjusted for the full set of controls listed in Table D4, and state fixed effects. The solid line presents the fitted linear relationship between the residuals of policy exposure and log mean hourly wages, and the green area is show the 95% confidence interval.

Panel (a) of Figure 11 plots mean hourly wages for municipal street laborers in 1928 by exposure quantile. While exposure rises sharply across quantiles, wages increase roughly linearly, indicating that cities more affected by the quotas paid higher wages to their lowest-skilled workers in the late 1920s. Panel (b) shows the partial correlation between exposure and wages after controlling for city-level covariates (listed in Table D4) and state fixed effects: a one-percentage-point increase in exposure is associated with roughly 0.6 percent higher wages, consistent with our DID estimates.[37]

[34] Table C3 reports the DID estimates for foreign-born shares and wages in the American Contractor sample.

[35] In most cases, a municipality would report one wage rate. When more than one wage rate is reported, we use the lowest wage, and when a range is reported, we use the bottom of the range.

[36] Table C4 provides information on the mean quota exposure measure and mean hourly street laborer wage in 1928 for the sample of cities and towns.

[37] It is useful to compare the results in Figure 11 to results from the pooled sample in Figure C1. That figure repeats the same exercise using the pooled wage sample, plotting pre- and post-1920 relationships between exposure

6.3.3 Long-Run Effects

Our analysis shows that more exposed labor markets experienced an immediate decline in immigration inflows and a corresponding rise in the wages of low-skilled workers that persisted in the 1920s. The central question of this subsection is whether the wage effects we document persisted over the longer run. Motivated by prior work highlighting adjustment processes in labor markets (Abramitzky et al., 2023; Clemens et al., 2018; Monras, 2020), we examine outcomes through 1940 to assess how exposed labor markets adapted and whether elevated wage premia for low-skilled workers remained as immigration restrictions persisted.

Table 5. Long-Run Effects of Exposure to Border Closure Policy on Low-Skilled Wages

Sample:	**Pooled** (1)	**Manufacturing & Mining** (2)	**American Contractor** (3)	**Construction Union** (4)	**Agriculture** (5)
A. Log Employment:					
1. *Overall:*					
Quota Exposure X (1920 < Year < 1940)	-0.759***	-0.950***	-0.653	-0.609	0.194
	(0.212)	(0.307)	(0.455)	(0.400)	(0.413)
Quota Exposure X (Year = 1940)	-1.488***	-1.721***	-1.112*	-1.580**	-0.463
	(0.315)	(0.466)	(0.574)	(0.725)	(0.549)
Pre-1920 Dependent Mean	6,021	4,042	9,865	14,144	3,986
2. *Laborers:*					
Quota Exposure X (1920 < Year < 1940)	-1.047***	-1.294***	-1.356*	-0.0712	0.360
	(0.279)	(0.360)	(0.736)	(0.489)	(0.450)
Quota Exposure X (Year = 1940)	-1.252***	-1.377***	-1.154	-1.172	-0.275
	(0.379)	(0.520)	(0.837)	(0.946)	(0.730)
Pre-1920 Dependent Mean	1,732	1,409	2,210	3,148	1,491
Observations	1,596	992	232	180	192
B. Log Mean Hourly Wage (Laborers):					
Quota Exposure X (1920 < Year < 1940)	0.656***	0.545***	0.655***	0.809***	0.527
	(0.148)	(0.138)	(0.198)	(0.269)	(0.339)
Quota Exposure X (Year = 1940)	0.572***	0.673***	0.214	0.911***	0.198
	(0.158)	(0.155)	(0.300)	(0.198)	(0.740)
Pre-1920 Mean Hourly Wage ($)	0.26	0.24	0.61	0.40	0.190
Observations	3,372	1,482	350	532	1,008

Notes: This table presents estimates of the interaction coefficients between quota exposure measure QE and the post-1920 and 1940 indicators. Column 1 presents results for the pooled sample, and columns 2-5 present results for each data source separately. In panel A, the outcome variable is the log total RLM-industry employment (overall and laborers). In panel B, the outcome variable is the log mean hourly wages for laborers for RLM-industry pair, using our collected wage data for 1910-1929 and the 1940 Census for 1940. All specifications include industry-year and RLM-industry fixed effects. Each RLM-industry pair has a varying number of wage observations from 1910-1929 and a 1940 wage observations calculated using the 1940 Census. Robust standard errors, clustered at the RLM-industry level in parentheses.. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

Panel A of Table 5 presents difference-in-differences estimates for log employment using full-count census data from 1910–1940. Exposure is associated with large and durable employment contractions. In the pooled sample, employment for the average RLM–industry pairs fell by roughly

and wages. Before 1920, there is no systematic relationship between quota exposure and wages, but after 1920 the slope becomes distinctly positive. The contrast between pre- and post-periods suggests that the positive relationship observed in 1928 among municipal laborers likely reflects the lasting effects of immigration restrictions rather than pre-existing wage differentials.

8 percent in the 1920s and by nearly 15 percent by 1940; the declines for laborers were about 10 and 12 percent, respectively. These patterns appear across datasets, with the sharpest drops in manufacturing and mining. Although internal migration and arrivals from non-restricted origins increased in more exposed areas (Abramitzky et al., 2023), these inflows did not fully compensate for the loss of workers, especially low-skilled, from restricted countries.

Panel B reports parallel DID estimates for log mean hourly wages of laborers, incorporating hourly earnings from the 1940 census. For the pooled sample, the wage coefficient is 0.656 in the 1920s and remains sizable at 0.572 in 1940, indicating that higher relative wages persisted as low-skilled labor remained scarce. Across datasets, the most pronounced and durable wage gains appear in manufacturing and mining, where exposure and labor shortages were most acute, while effects in construction and agriculture are smaller and appear to moderate by 1940.[38]

6.4 Robustness

We assess the robustness of our findings to alternative exposure definitions, the inclusion of additional controls, different levels of aggregation, and wartime disruptions. We report the full results with detailed descriptions in Online Appendix D. We first construct a state-level exposure measure, aggregating pre-quota immigrant composition to the state level rather than the RLM–industry unit used in the baseline. This exercise ensures that the results are not driven by how local labor markets and exposure measures are defined. The estimated effects remain positive and significant, indicating that the main findings are not sensitive to the geographic unit of analysis. Using a binary indicator for high versus low exposure and redefining exposure with 1900 instead of 1910 immigrant shares yields similar results. We next add a broad set of pre-1910 demographic and industrial controls, including the foreign-born share, total population, manufacturing and farming employment, to account for pre-existing economic differences across markets. The inclusion of these covariates leaves the magnitude and significance of the coefficients virtually unchanged. We then examine sensitivity to aggregation by collapsing the data to the RLM or industry level, finding consistent results across both. Finally, incorporating a World War I exposure measure confirms that temporary wartime disruptions do not explain the observed post-1920 wage patterns.

7 Composition Effects

We investigate whether the observed wage increases reflect genuine changes in labor market wages or are instead driven by changes in the composition of low-skilled workers. We proceed in three steps. First, we consider whether spillovers across local markets could bias the estimates. Second, we assess the scope for sample selection arising from differential employment responses. Finally, we quantify the principal composition channel—changes in the immigrant share—using historical immigrant wage penalties to bound the magnitude of its effect.

[38]Interpretation of the 1940 coefficients should be cautious, as they follow the Great Depression and early wartime mobilization, and wages in the 1940 full-count census are not perfectly comparable to our pre-1940 wage data.

Possible SUTVA Violations. A potential concern is that migration across labor markets may have altered the composition of workers between low- and high-exposure RLM–industry pairs, violating the Stable Unit Treatment Value Assumption (SUTVA; see Angrist et al. (1996)). If workers moved from low- to high-exposure areas in response to rising wages, observed effects could partly reflect changes in worker composition rather than true wage adjustments. To examine this, we conduct two tests. First, we re-estimate the baseline specification after excluding low-exposure RLM–industry pairs, ensuring that our results are not influenced by markets least affected by the quotas. Second, we borrow measures of out-migration from Abramitzky et al. (2023) and identify areas sending a high share of internal migrants to strongly exposed markets. We then exclude (i) RLM–industry pairs in these high-out-migration areas and (ii) the state with the highest out-migration rate within each census division. Across all specifications, the estimated wage and immigrant-share effects remain virtually unchanged, suggesting that migration from low- to high-exposure markets—and potential SUTVA violations more broadly—are not empirically important. Full results are presented in Online Appendix E.

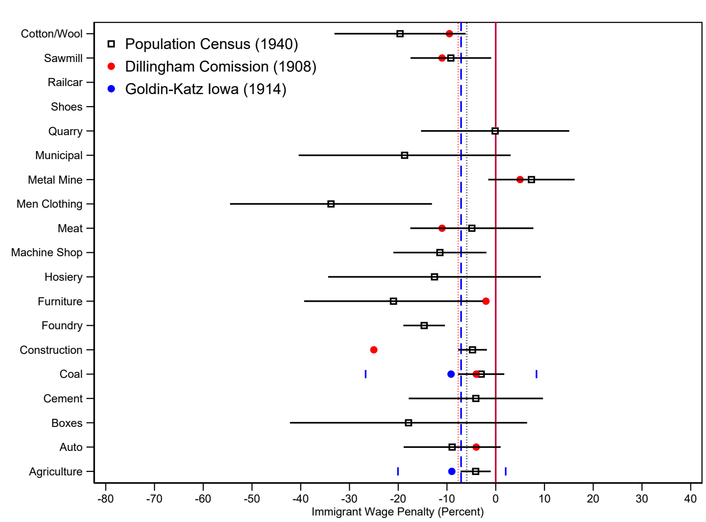
Sample Selection. We next examine whether, and to what extent, sample selection within the wage data could affect the estimated effects. Using full-count census data, we compare the characteristics of laborers in RLM–industry pairs included in the wage sample with those excluded, as well those in high- vs. low-exposure markets, tracing how these characteristics evolved over time.[39] The descriptive evidence reveals modest differences before 1920: RLM–industry pairs in the wage sample and those with higher exposure were somewhat more urban and more affected by immigration than those excluded or with low exposure. The main compositional change between the pre- and post-1920 periods is the decline in the immigrant share. In addition, the share of Black workers rises slightly in the wage and higher-exposure samples—an adjustment that would tend to attenuate our estimated effects—while the average age of workers increases slightly.

We then formally test for selection bias using inverse probability weighting (IPW) and a selection correction model in the spirit of Heckman (1979). The IPW estimates reweight observations by their probability of inclusion in the wage sample based on individual and RLM-industry pairs characteristics, while the pseudo-Heckman specification explicitly models the selection process through a two-step correction process. Both approaches reveal mild negative selection: RLM–industry pairs more likely to be included in the wage data, and those more likely to fall into the high-exposure group within the sample, tend to have somewhat lower unobserved wage components and are generally more urban and immigrant-intensive. Adjusting for these differences leaves the estimated coefficients virtually unchanged. Selection is present in levels—both between included and excluded markets and within the sample across exposure groups—but not in trends over time, suggesting that selection does not seem to bias our difference-in-differences estimates. A detailed discussion and full results are provided in Online Appendix E.

[39]See Table E2 in Online Appendix E for more details.

Immigrant Wage Penalties and Composition Effects. The preceding analyses show that neither spillovers across labor markets nor sample selection meaningfully bias the estimated effects of quota exposure. The principal remaining composition concern arises from the sharp decline in the share of recent immigrants from quota-restricted countries. Because immigrants earned lower wages than natives during this period, a reduction in the foreign-born share could mechanically increase mean wages even if underlying wage schedules remained unchanged. We therefore summarize historical evidence on immigrant wage penalties and quantify how much of the estimated wage effects could plausibly be attributed to these compositional shifts. Full data descriptions, regression specifications, and calculation steps are provided in Online Appendix E.

Figure 12. Immigrant Wage Penalty Estimates by Industry



Notes: The figure plots the industry-specific immigrant wage penalties reported by the Dillingham Commission (1908) in red (USIC,1911), estimates based on 1940 Population Census in black squares, and the Iowa State Census Sample in blue (Goldin and Katz, 2000). The 1940 population Census and Goldin-Katz Iowa estimates are the coefficients on a recent immigrant indicator from a regression where the outcome is an individual log mean hourly wage and additional demographics, in addition to industry and county fixed effects. The vertical bars show 95% confidence intervals.

To establish the relevant wage differentials, we rely on three historical data sources. First, employer payroll records collected by the Dillingham Commission (1907–1911) show that foreign-born male workers earned on average 9.3 percent less than natives across the eight industries overlapping with our sample, with substantial industry variation as shown by the red circles in Figure 12. Second, individual-level data from the 1915 Iowa State Census yield immigrant wage penalties of 7–9 percent for male laborers after adjusting for demographic characteristics as shown by the blue circles in Figure 12. Finally, estimates from the 1940 Population Census, the first decennial census to report wages, indicate a similar penalty of about 9 percent across all 21 industries in our analysis, with considerable heterogeneity as shown by the black squares in Figure 12.[40]

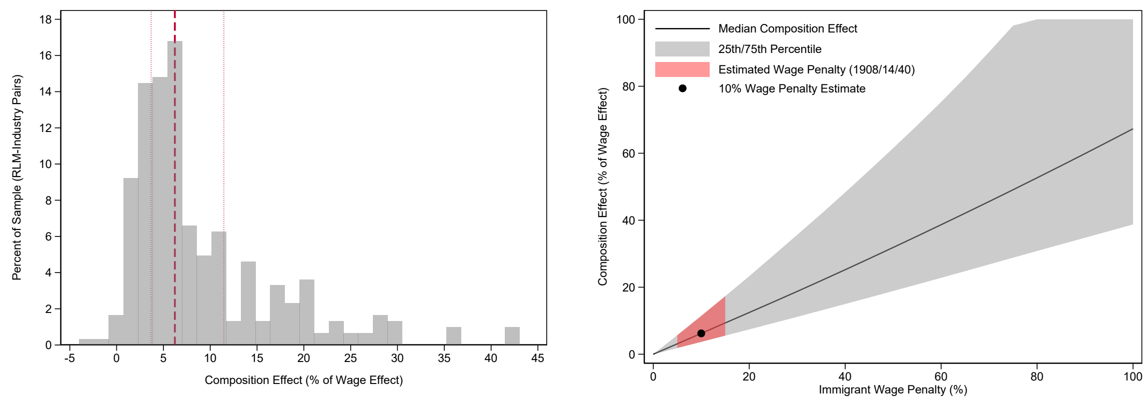
Using these wage-penalty estimates, we adapt the approach of Ager et al. (2024) to gauge how

[40]Despite being measured later, the 1940 evidence aligns closely with the pre-1920 sources and provides broad industry coverage.

Figure 13. Composition Effects Analysis

(a) 10% Wage Penalty Distribution

(b) Varying Wage Penalties



Notes: Panel (a) plots the histogram of the percent contribution of composition shifts to the overall estimated effect of immigration restrictions on log mean hourly wages under the assumption of a constant and uniform 10 percent immigrant wage penalty. The dashed red line shows the value for the median RLM-industry pair, and the thin dotted red lines present the 25th and 75th percentiles. Panel (b) presents the back-of-the-envelope distribution of composition effects for varying immigrant wage penalties. The black line plots the median composition effect for each wage penalty, the gray area shows the 25th and 75th percentiles of the composition effect, and the red area highlights the estimated composition effects that match our estimates of immigrant wage penalties from Figure 12.

much declining immigrant shares could explain rising wages in high-exposure markets. First, we predict post-1920 recent-immigrant or foreign-born shares for each RLM–industry pair using its quota-exposure measure and the estimated effects in column 1 of Table 2. Second, we assume a constant 10 percent immigrant wage penalty based on Figure 12. Third, we recover pre-1920 native- and foreign-born mean hourly wages using pre-1920 mean wages and immigrant shares. Fourth, we construct the implied post-1920 mean wage if only the composition of workers changed and pre-1920 wages remained fixed. Fifth, we calculate the composition-only wage growth as the ratio of this implied post-1920 wage to the pre-1920 wage. Sixth, we compute total implied wage growth due to the quotas by multiplying each exposure measure by the pooled DID estimate in column 1 of Table 4. Finally, we take the ratio of composition-only growth to total implied growth to obtain the share of the wage effect attributable to composition for each RLM–industry pair.

Panel (a) of Figure 13 shows that, under a 10 percent immigrant wage penalty, compositional shifts account for only a small share of the post-1920 wage increase: the median effect is just under 10 percent, and Table E4 indicates that composition explains only 6–9 percent of total wage gains in the median RLM–industry pair, with over two-thirds of pairs below 10 percent and more than 90 percent below 25 percent. Panel (b) varies the assumed penalty from 0 to 100 percent and shows that composition effects rise mechanically with higher penalties but only fully match the estimated wage response at values above roughly 80 percent—well beyond any empirically credible estimates from the Dillingham Commission, the Iowa State Census, or the 1940 Census—indicating that within the plausible range, composition effects remain modest.

8 Mechanisms

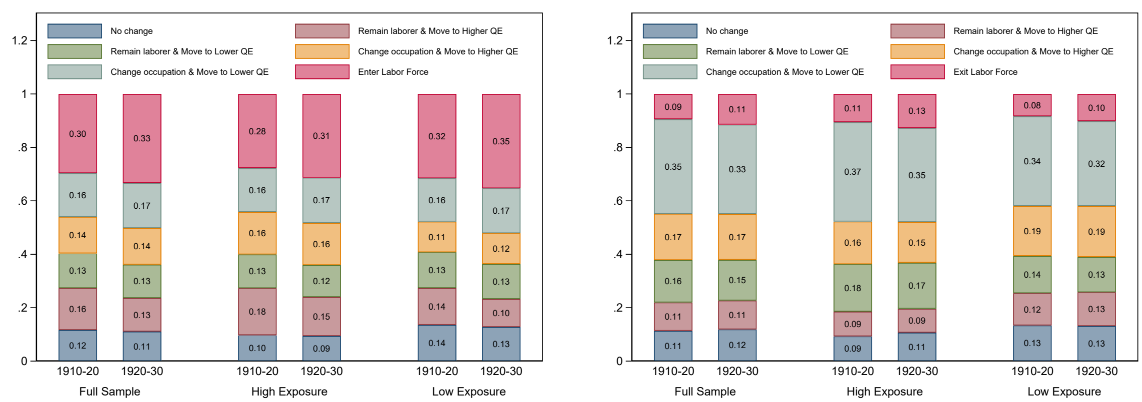
Our results show that high-exposure RLM–industry pairs experienced large and persistent increases in mean hourly wages, with considerable heterogeneity across sectors. The theoretical framework in Section 3 highlights two key margins of adjustment to an immigration-driven labor supply shock: labor mobility and capital adjustment. In the model, wages remain elevated in the medium run when mobility responses are limited and capital adjusts slowly or unevenly across sectors. In this section, we examine these two channels empirically.

8.1 Labor Mobility

A central question is whether domestic migration offset the loss of foreign-born labor. Prior work emphasizes internal migration as a key adjustment margin to immigration shocks, particularly through inflows of Black domestic migrants and immigrants from unrestricted origins (Abramitzky et al., 2023; Price et al., 2020). However, these studies generally consider the workforce as a whole, whereas the quotas primarily affected *low-skilled laborers*, the focus of our analysis.

Figure 14. Entry and Exit Patterns of Laborers - Pre and Post-Restrictions

(a) Entry Rates (Laborers in $T+10$) (b) Exit Rates (Laborers in T)



Notes: This chart presents average rates of entry and exit for laborers in RLM-industry pairs based on linked samples from 1910-1920 and 1920-1930. Panel (a) shows the distribution of origins for individuals who were laborers in year $T+10$, tracking their status in year T. Panel (b) shows the change patterns for individuals who were laborers in year T, tracking their status in year $T+10$. Results are shown for the full sample, above median quota exposure RLM-industry pairs, and below median quota exposure RLM-industry pairs. Each RLM-industry pair is observed for two time periods (1910-1920 and 1920-1930), resulting in 798 observations across 399 RLM-industry pairs.

To examine low-skilled mobility directly, we use the IPUMS Linked Representative Samples for 1910–1920 and 1920–1930 (Ruggles et al., 2025). These data allow us to follow individuals across censuses and construct worker-level transitions for males aged 16–65 who were laborers at time T. We observe their occupation, labor-force status, and location ten years later, enabling measurement of *entry* (not a laborer in T, laborer in $T+10$) and *exit* (laborer in T, not a laborer in $T+10$) flows by exposure level.

Figure 14 summarizes these transitions. Panel (a) shows entry rates for individuals observed as laborers in $T+10$; Panel (b) shows exit rates for those employed as laborers in T. Each bar decomposes outcomes into six categories: no change, remaining a laborer while moving to higher or lower exposure markets, changing occupations while moving to higher or lower exposure markets, or entering/exiting the labor force. A decline in moves to higher-exposure areas or in occupational switching after 1920 would imply reduced geographic and occupational mobility, consistent with localized wage adjustment rather than migration.

Panel (a) of Figure 14 shows that entry rates changed little after 1920: overall labor-force entry rose slightly, but moves into high-exposure markets fell, indicating weaker internal migration. Most new entrants appear to have been nearby non-participants drawn in by higher local wages. Mobility patterns across exposure groups show similarly limited movement: workers in high-exposure markets became marginally more likely to stay put, while those in low-exposure markets showed only small increases in moves toward higher-exposure areas and decreases in moves toward lower-exposure areas.

Panel (b) of Figure 14 shows that exit patterns likewise exhibit stability over time and across exposure levels. The shares of workers remaining in the same RLM–industry pair or exiting the labor force rose slightly after 1920, while moves to lower-exposure markets declined modestly. Splitting by exposure shows similar behavior: high-exposure markets saw a small rise in "no-change" outcomes, and low-exposure markets experienced minor shifts toward remaining laborers and moving to higher-exposure areas. Across all groups, occupational switching away from laborer declined, suggesting stronger incentives for workers to remain in place as local labor demand increased.[41]

Taken together, the linked-sample evidence demonstrates that internal migration did not offset the labor-supply shock induced by the quotas for low-skilled workers. High-exposure markets did not attract more laborers nor experience elevated outflows; instead, workers remained in place and benefited from rising wages. Such limited mobility is one of the factors associated in our model with persistent local wage effects following a negative labor supply shock.

8.2 Capital Adjustment

We next assess whether firms responded to rising wages by substituting toward capital. In our theoretical model, capital deepening reduces labor demand and attenuates wage effects, but the magnitude and speed of adjustment depend on adjustment costs, which differ across industries. When capital is slow to adjust or responds more in some sectors than others, wage effects remain elevated and heterogeneous across industries.

Manufacturing. To study capital adjustment in manufacturing, we use city-by-industry data from the U.S. Census of Manufactures (1910–1930), which report employment, capital value, and

[41] Table C5 presents the formal estimates. Entries show no systematic relationship with quota exposure, and exit rates fall significantly with exposure—driven primarily by reductions in occupational switching away from laborer. This suggests stronger worker *retention* in high-exposure markets after 1920. Mobility toward lower- or higher-exposure markets declines slightly in both periods, further indicating that geographic adjustment was limited.

horsepower—a widely used measure of capital in early-twentieth-century production. Capital data are available for 158 RLM–industry pairs out of the 248 in our Manufacturing and Mining Sample.

Column 1 of Panel A of Table 6 shows that the wage effects of quota exposure remain large when the sample is restricted to manufacturing industries with available capital data. Column 1 of Panel B indicates a positive, though imprecisely estimated, effect on log horsepower per worker. To assess heterogeneity, we split industries by their pre-1920 capital–labor ratios (columns 2–3) and horsepower–labor ratios (columns 4–5). In both splits, the estimated effects on horsepower per worker are an order of magnitude larger in initially less-mechanized industries, while more-mechanized industries—despite experiencing the largest wage increases—exhibit little capital adjustment. In contrast, wage effects are strongest in the initially more-mechanized industries.

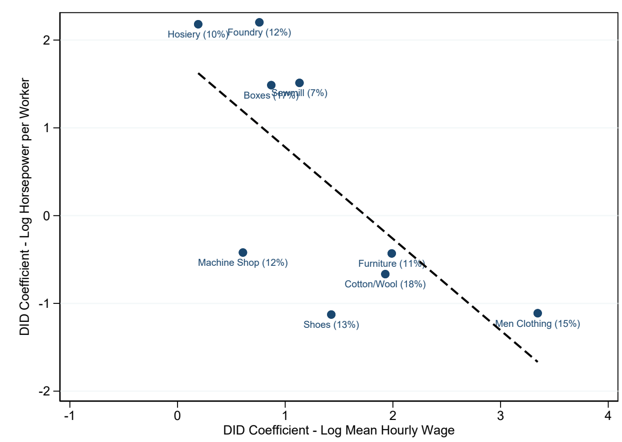
Table 6. Quota Exposure and Capital Adjustment - Manufacturing

		Capital-Labor Ratio (Pre-1920)		**Horsepower-Labor Ratio (Pre-1920)**	
	Full Sample	**Above Median**	**Below Median**	**Above Median**	**Below Median**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
A. Log Mean Hourly Wage					
Quota Exposure X Post 1920	0.720***	0.980***	0.382	1.363***	0.105
	(0.252)	(0.326)	(0.364)	(0.318)	(0.338)
Pre-1920 Dependent Mean	0.24	0.25	0.24	0.23	0.26
Observations (RLM-Industry Pair)	734	455	279	451	283
B. Log Horsepower per Worker					
Quota Exposure X Post 1920	1.043	0.785	1.960	1.036	2.431
	(0.849)	(1.039)	(1.458)	(1.037)	(1.471)
Pre-1920 Dependent Mean	3.42	3.42	3.43	5.10	0.67
Observations (RLM-Industry Pair)	590	388	202	390	200
Quota Exposure - Mean	0.11	0.11	0.11	0.09	0.13
Horsepower-Labor Ratio Mean	1.90	2.11	1.55	2.90	0.30
Capital-Labor Ratio Mean	1,814	2,228	1,139	2,131	1,309
RLM-Industry Pairs	158	93	65	94	64

Notes: This table presents the effect of quota exposure on manufacturing wages and capital intensity using data from the Census of Manufacturers. Panel A shows difference-in-differences estimates for log mean hourly wages. Panel B shows difference-in-differences estimates for log horsepower per worker. Column 1 presents results for the full sample. Columns 2-3 split the sample by above/below median capital-labor ratio (pre-1920). Columns 4-5 split the sample by above/below median horsepower-labor ratio (pre-1920). All estimates are coefficients of the interaction between quota exposure (QE) and a post-1920 indicator. All specifications include year and RLM-industry fixed effects, and industry-year fixed effects. Robust standard errors, clustered at the RLM-industry level, in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Figure 15 illustrates this negative relationship: industries experiencing the largest wage effects exhibit the smallest increases in capital intensity following quota exposure. Rather than responding through rapid capital deepening, these sectors appear to absorb the labor supply shock primarily through higher wages. This pattern suggests limited substitutability between capital and labor in already capital-intensive industries. It is consistent with our theoretical model in which adjustment costs are higher for sectors operating with substantial fixed capital, where existing production technologies, capacity constraints, or costly installation and retooling raise the marginal cost of additional capital investment. As a result, these industries face greater frictions in adjusting their capital stock and substitute away from labor more slowly, leading wage responses to play a more prominent role in equilibrating labor markets.

Figure 15. Heterogeneous Wage and Capital Adjustments by Industry



Notes: This figure plots the relationship between industry-specific wage effects and capital adjustment responses to immigration restrictions. The x-axis shows DID coefficients for log mean hourly wage from the quota exposure analysis. The y-axis shows DID coefficients for log horsepower per worker, measuring capital intensity adjustment. Each point represents an industry, labeled with its name and the percentage of immigrant workers in parentheses. The dashed line shows the fitted relationship from a regression of capital adjustment on wage effects. Capital data are from the Census of Manufacturers.

Agriculture. We complement the manufacturing analysis with state-level data from the Censuses of Agriculture (1910, 1920, 1925, 1930) (Haines et al., 2018). These data allow us to track mechanization—via machinery value, draft animals, tractors, motors, and equipment—across agricultural states with varying exposure to immigration restrictions. Consistent with prior evidence on labor scarcity and mechanization (e.g., Abramitzky et al. 2023, Clemens et al. 2018; San 2023), Panel A of Table 7 shows substantial increases in machinery value per acre and declines in horses and mules per acre in more exposed states, alongside increases in improved farmland and average farm size. Unlike some other historical episodes of labor scarcity, we do not find clear evidence of crop switching. Panel B shows that post-1920 machinery adoption (tractors, gas engines, electric motors, equipment expenditures) is positively correlated with exposure, consistent with substantial capital substitution when labor became scarce.

Taken together, across both manufacturing and agriculture, firms responded to the immigration restrictions by increasing capital intensity, but adjustment was uneven. Capital deepening was most pronounced in initially low-mechanized sectors, while mature, capital-intensive industries showed little response—precisely where we find the largest and most persistent wage effects. Combined with the limited internal migration documented above, these patterns match the medium-run dynamics emphasized in our theoretical model: when worker mobility is low and capital adjusts slowly and asymmetrically across sectors, immigration restrictions generate localized and durable increases in the demand for low-skilled labor, leaving lasting wage gains rather than quickly reverting to pre-shock wage levels.

Table 7. Quota Exposure and Capital Adjustment - Agriculture

Sample:	**Estimate** (1)	**Std. Error** (2)	**Pre-1920 Mean** (3)	**Observations** (4)
A. DID Outcomes				
Value of Implements and Machinery, per 1000 acres	8,284**	(3,506)	3,001	192
Horses and Mules, per 1000 acres	-28.77***	(9.641)	27.33	192
Share Improved Farmland	1.067**	(0.510)	0.51	192
Share Corn and Hay Crops (Labor Intensive)	0.020	(0.055)	0.19	192
Share Cereal and Wheat Crops (Capital Intensive)	0.027	(0.048)	0.09	192
Average Farm Size	460	(336)	198	192
B. Post-1920 Outcomes (per 1000 acres)				
Tractors	4.179**	(1.994)	0.83	96
Autos	4.490	(8.709)	5.22	96
Trucks	12.23**	(5.375)	1.55	96
Electric Motors	10.03***	(2.781)	0.73	48
Gas Engines	9.244**	(3.519)	1.46	48
Expenditures on equipment	4,984***	(1,846)	863	48

Notes: This table presents the effect of quota exposure on agricultural capital investment and mechanization. Panel A shows DID estimates of the interaction between quota exposure QE and a post-1920 indicator. Panel B presents cross-sectional estimates of quota exposure on post-1920 outcomes. Column 1 shows the estimate (DID coefficient in Panel A, OLS coefficient in Panel B), column 2 shows robust standard errors clustered at the RLM-industry level, column 3 shows pre-1920 means of the dependent variable, and column 4 shows the number of observations. All specifications include year and RLM-industry fixed effects. Robust standard errors, clustered at the RLM-industry level, in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

9 Conclusion

The 1920s national-origins quotas created a large and uneven contraction in the supply of low-skilled immigrant labor across U.S. regional labor market–industry cells. Using newly digitized annual wage data for laborers across multiple sectors, we show that areas historically more reliant on immigrants from the newly restricted countries experienced substantially faster wage growth after 1920. These wage effects emerged quickly, intensified through the mid-1920s, and remained sizable in the medium run, even as labor markets adapted. On average, more exposed markets experienced wage increases on the order of 6–8 percent—magnitudes consistent with benchmark elasticity estimates and concentrated in the sectors that had depended most heavily on immigrants.

Our long-run evidence suggests that these wage gains reflected persistent labor scarcity rather than short-lived adjustment. Full-count census data show that employment fell more sharply in more exposed markets and industries, with the largest and most durable contractions in manufacturing and mining. Although internal migration and immigration from unrestricted countries increased, these inflows were too small to offset the loss of workers from restricted origins. At the same time, capital substitution and changes in production technologies emerged selectively, concentrated in sectors facing the steepest declines in immigrant labor. This pattern of incomplete labor replacement and uneven adjustment helps explain both the magnitude and the persistence of the wage response to the quotas.

While much of the immigration literature studies wage effects during periods of rising inflows, we show that large declines in immigrant labor can produce meaningful and persistent wage gains when internal mobility is limited and capital adjustment is costly. Because these margins of adjustment are intrinsic to labor markets confronting immigration reductions, the medium-run patterns we document are informative for current policy debates. Our findings suggest that immigration restrictions targeting low-skilled workers can materially raise local wages in the short and medium run, especially in sectors historically dependent on immigrant labor, though the ultimate effects depend on the speed of migration and capital substitution.

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For Online Publication

Online Appendix to

"Immigration Restrictions and the Wages of Low-Skilled Labor: Evidence from the 1920s"

Jeff Biddle Elior Cohen

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A Data Sources, Sample Construction, and Exposure Measures

This appendix provides additional detail on the historical sources, data construction, and exposure measures used in the analysis.

A.1 Quota Intensity and War Restrictions

Table A1. Country and Region-Specific Quota Intensity and War Restriction Variables

Country or Region	**Quota Intensity Measure (1)**	**WWI Restriction Measure (2)**
A. High-Restriction Countries:		
Asia	0.95	0.50
Central Europe	0.97	0.98
Eastern Europe	0.94	0.96
Greece	0.97	0.50
Italy	0.96	0.89
Portugal	0.95	0.41
Rest of World	0.69	0.00
Russia	0.93	0.95
Spain	0.98	0.14
B. Low-Restriction Countries:		
Germany	0.00	0.92
Ireland	0.00	0.79
Scandinavia	0.10	0.68
United Kingdom	0.00	0.80
Western Europe	0.56	0.72
C. No-Restrictions Countries:		
Canada	0.00	0.00
Caribbean	0.00	0.11
Latin America	0.00	0.00
Mexico	0.00	0.00

Source: Abramitzky et al. (2023). Notes: The quota intensity measure for a region equals the predicted unrestricted 1920s inflow minus the number of quota slots, divided by the predicted unrestricted inflow. The WWI restriction measure is constructed in a similar way using wartime disruptions.

Exposure measures to immigration restrictions used in the analysis are constructed using quota intensity indices QI_r that capture how strongly the 1920s quota system restricted immigration from different sending regions. These indices come from Abramitzky et al. (2023), who estimate counterfactual "unrestricted" immigration flows for each region using a pre-1914 fitted polynomial on immigration flows from each sending region and then compare them to the number of quota slots allocated under the new legislation.

Table A1 reports the quota intensity and World War I (WWI) restriction indices by broad sending region. The quota intensity for region r is defined as:

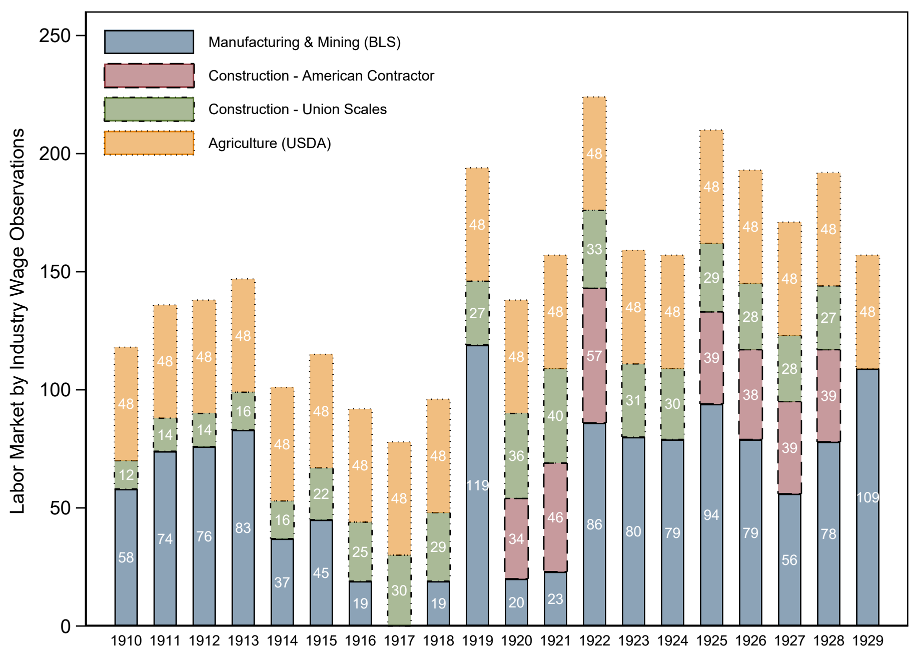
$$QI_r = \frac{\text{unrestricted flow}_r - \text{quota slots}_r}{\text{unrestricted flow}_r},$$

so that $QI_r = 0$ when quotas do not bind and $QI_r = 1$ when flows are fully shut down.

We similarly construct a WWI exposure measure w_c using the WWI restriction indices in column 2 of Table A1. This measure is highly correlated with q_c (correlation coefficient 0.87) and is used in robustness checks that control explicitly for wartime disruptions to migration flows.

A.2 Archival Sources and Coverage

Figure A1. Wage Observations by Labor Market–Industry Dataset, 1910–1929



Notes: The figure reports the number of labor market–industry wage observations per year by dataset. Manufacturing & mining (BLS) are from employer payrolls; construction (American Contractor) from contractor reports of city-level wages; construction (BLS union scales) from negotiated minimums; agriculture (USDA) from state daily/monthly wages converted to hourly (10 hrs/day).

We assemble annual wage data for low-skilled workers in the United States between 1910 and 1929 from four main historical sources: (i) industry-specific surveys of manufacturing and mining wages conducted by the U.S. Bureau of Labor Statistics (BLS), (ii) wage reports from the trade journal *American Contractor*, (iii) BLS surveys of construction trade union wage scales, and (iv) surveys of agricultural wages conducted by the U.S. Department of Agriculture (USDA). These sources provide repeated observations on narrowly defined low-skilled occupations—typically labeled "laborers"—for a variety of local labor markets and industries.

The basic description of what we call our pooled sample is found in the text. To show how the number of wage observations in this sample evolves over time and across sources, Figure A1 plots the annual number of wage observations by dataset for 1910–1929. Coverage is relatively sparse before World War I, increases during the 1910s, and peaks in the mid- to late 1920s, reflecting the expansion of BLS and USDA reporting and the introduction of the *American Contractor* survey.

Manufacturing and Mining Wage Surveys. The BLS industry surveys report mean wages for detailed occupations within individual manufacturing and mining industries. The unit of observation is either a state–industry or a city–industry cell, depending on how BLS summarized the underlying establishment-level returns in a given year.

We focus on the lowest-paid occupation reported in each industry, which is usually labelled "laborer" or an equivalent low-ranked job title. For a small number of industries where no explicit "laborer" occupation appears, we instead select a large occupation at the bottom of the wage distribution (for example, doffers in cotton textiles). In coal mining we retain three distinct low-skilled occupations—pick miners, inside-mine laborers, and outside-mine laborers. Table A2 lists the industries, occupations, and years covered in the manufacturing and mining sample, together with information on whether semi-skilled wage data are also available for the industry.

Construction Wages: Contractors and Unions. As described in the text, our data on the construction sector combines information from the journal *American Contractor* with BLS surveys of union wage scales in construction. From *American Contractor* we use reports for October 1920, October 1921, May 1922, October 1922, June 1925, October 1925, May 1926, September 1926, April 1927, July 1927, May 1928, and October 1928. When a city appears more than once in a calendar year, we keep the highest reported wage so that each city–year cell corresponds to a single wage. This yields 294 wage observations in 61 cities. The BLS union wage scale surveys report information for cities where at least half of workers in an occupation were covered by a union agreement. Included are the hourly union scales for "building laborers." These union scales represent minimum pay rates and may understate actual earnings when workers receive premia above the contractual minimum. Nonetheless, they provide broad geographic and temporal coverage of low-skilled construction wages. Table A3 lists the cities covered by each construction source and flags whether a city appears in the wage survey, in the union scale data, or in both. Many large cities appear in both sources, while a set of smaller towns appear only in the contractor survey. For both construction datasets, the RLM is defined as the county containing the city in which wages are reported, and exposure is measured using the county-level quota exposure index q_c defined in Equation 7 in the main text.

Table A2. Datasets Scope and Coverage

	1950 Industry Code (1)	Observations (2)	RLMs (3)	Years Covered (4)	Semi-Skilled Wages (5)
A. Manufacturing and Mining:					
Auto: laborers	376	28	7	1919/22/25/28	Yes
Boxes: laborers	457	17	11	1919/25	
Cement: laborers	317	4	4	1929	
Coal: In-mine laborers	216	48	10	1919/22/24/26/29	Yes
Coal: pick miners	216	43	9	1919/22/24/26/29	Yes
Coal: Outside of mine laborers	216	48	10	1919/22/24/26/29	Yes
Cotton Textiles: doffers	439	99	12	1911–14/16/18/22/24/26/28	Yes
Foundries: laborers	336–338, 346–348	123	28	1919/23/25/27/29	Yes
Furniture: laborers	309	41	17	1915/19/29	
Hosiery & Underwear: "other occ."	436, 446, 449	73	16	1910–14/22/26/28	
Iron Mines: laborers, outside mine	206	3	3	1924	
Machine Shops: laborers	356–358, 367	123	28	1919/23/25/27/29	Yes
Meat Packing: laborers (maint. & repair)	406	5	1	1921/23/25/27/29	Yes
Men's clothing: basters (coat)	448	59	7	1911–14/19/22/24/26/28	Yes
Metal Mining: laborers (outside mine)	206	8	8	1924	
Mill Work (Wood): laborers	307–308	66	12	1910–13/15/19	
Quarries: laborers	236	4	4	1929	
Railcar Manufacture: laborers	379	63	17	1910–13	
Sawmills: laborers	307–308	211	23	1910–13/15/19/21/23/25/28	Yes
Shoes: cutters, trimming hand	488	81	12	1910–14/20/22/24/26/28	Yes
Woolen Textile: dyehouse laborers	439	87	9	1910–14/16/18/20/22/24/26/28	Yes
B. Construction:					
American Contractor Dataset	246	294	61	1920–22, 25–28	Yes
BLS Construction Union Wage Scales	246	487	45	1910–28	
C. Agriculture:					
USDA Farm Laborer Wage Dataset	105	960	48	1910–29	
Observations				2,973	
Number of Relevant Labor Markets (RLMs)				125	
Number of Industries				18	
Number of RLM–Industry Pairs				399	

Notes: The table lists industries and occupations in the main panel, grouped by dataset. Column (4) reports years with wage observations (with subsequent years abbreviated). The last rows summarize the size of the pooled panel.

Table A3. American Contractor Wage Survey and Construction Union Wage Scales: City Coverage

City (1)	Wage Survey (2)	Union Wage (3)	City (4)	Years Covered (5)	Union Wage (6)
Akron, OH	YP	N	New Haven, CT	YP	Y
Alliance, OH	YP	N	New Orleans, LA	N	Y
Atlanta, GA	YF	Y	New York, NY	YF	Y
Baltimore, MD	YF	Y	Newark, OH	YP	N
Binghamton, NY	YP	N	Norfolk, VA	YF	N
Boston, MA	YF	Y	Omaha, NE	YF	Y
Bridgeport, CT	N	Y	Peoria, IL	N	N
Buffalo, NY	YF	Y	Philadelphia, PA	YF	Y
Butte, MT	N	Y	Pittsburgh, PA	YF	Y
Chicago, IL	YF	Y	Portland, OR	N	Y
Cincinnati, OH	YF	Y	Portland, ME	N	Y
Cleveland, OH	YF	Y	Providence, RI	N	Y
Columbia, SC	YP	N	Raleigh, NC	YP	N
Columbus, OH	YF	Y	Reading, PA	YF	N
Dallas, TX	N	Y	Redfield, SD	YP	N
Dayton, OH	YF	N	Richmond, IN	YP	N
Denver, CO	N	Y	Richmond, VA	YF	N
Des Moines, IA	YP	Y	Rochester, NY	YP	Y
Detroit, MI	YF	Y	Saginaw, MI	YP	Y
Dubuque, IA	YP	N	Salt Lake City, UT	N	Y
Duluth, MN	YP	N	San Francisco, CA	YP	Y
Erie, PA	YF	Y	Savannah, GA	YP	N
Fairmont, WV	YP	N	Scranton, PA	N	Y
Fitchburg, MA	YP	N	Seattle, WA	N	Y
Flint, MI	YP	N	Sharon, PA	YP	N
Grand Rapids, MI	YF	Y	Shreveport, LA	YP	N
Greensboro, NC	YP	N	Sioux City, IA	YF	N
Houston, TX	N	Y	Spokane, WA	N	Y
Indianapolis, IN	YF	N	St. Joseph, MO	YF	N
Kansas City, MO	N	Y	St. Louis, MO	YF	Y
Kent, OH	YP	N	St. Paul, MN	N	Y
Lansing, MI	YP	N	St. Petersburg, FL	YF	N
Lima, OH	YP	N	Toledo, OH	YP	Y
Little Rock, AR	YP	Y	Warren, OH	YP	N
Los Angeles, CA	N	Y	Washington, DC	YF	Y
Louisville, KY	YF	Y	Webster City, IA	YP	N
Memphis, TN	YF	N	Wichita, KS	N	Y
Milwaukee, WI	YF	Y	Youngstown, OH	YF	N
Minneapolis, MN	N	Y			

Notes: "Wage Survey" refers to the *American Contractor* construction wage survey (1920–22, 1925–28); "Union Wage" refers to the BLS survey of construction trade union wage scales (1910–28). YF = city observed in all contractor survey years; YP = city observed in some contractor survey years; N = no observations in that source.

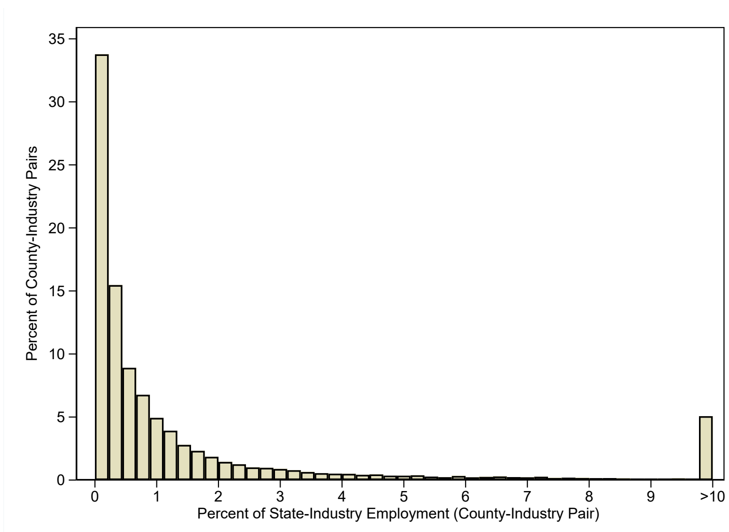
Agricultural Wages. Agricultural wages come from USDA publications that report average daily and monthly wages for farm laborers without board by state (USDA,1943). We use all 48 contiguous states and convert daily wages into hourly rates by assuming a 10-hour workday, which is consistent with contemporary accounts of farm work hours. The resulting series span 1910–1929 and form a balanced panel of 960 state–year observations.

Municipal Laborers (1928). The BLS municipal wage survey in 1928 collected information on the hourly wage paid to entry-level street workers—typically unskilled laborers employed to clean or repair streets—in more than 2,600 municipalities. In most cases, a single wage is reported per city; when multiple rates appear or a range is given, we use the lowest wage to capture the pay of the least experienced workers.

1940 Census Wages and Demographics. We also use the 1900–1940 Population Censuses to construct measures of long-run wages and local labor market characteristics at the RLM–industry level (Ruggles et al., 2024). For 1940 we compute hourly wages for low-skilled workers in each RLM–industry pair by dividing annual earnings by weeks and hours worked. We also use information on foreign-born shares and other demographic and socioeconomic variables from ICPSR's Historical, Demographic, Economic, and Social Data of the United States (Haines et al., 2010).

A.3 Relevant Labor Markets (RLMs) and Exposure to Immigration Restrictions

Figure A2. Distribution of County–Industry Employment Shares



Notes: The figure shows the distribution of county–industry 1910 employment shares across all state–industry pairs in the sample. The x-axis reports the percent of total state–industry employment accounted for by each county–industry pair, while the y-axis shows the percent of such pairs in each bin. Most county–industry pairs represent less than 1% of total state–industry employment, indicating a highly concentrated employment distribution.

A central empirical challenge is to map each our wage observation onto the correct local labor market — the geographic area that supplied the low-skilled workers whose wages the observation reflects. As described in the text, we do this by defining the *Relevant Labor Market* (RLM) for a given wage observation w_{ijt} as the geographic area that supplies low-skilled labor to the establishments that underlie the BLS mean wage for industry i in jurisdiction j at time t. Where the BLS reports wages at the city level, the RLM is simply that county; where BLS reports are at the state–industry level, the RLM aggregates county conditions using a set of industry employment weights described in the main text.

Figure A2 provides empirical justification for this choice: the figure plots the within-state distribution of the 1910 industry employment across counties for the state-industry pairs in our sample and shows the geographic distribution employment was highly uneven: a small number of counties typically accounted for a large share of total industry employment.

To gauge how the exposure measure relates to the underlying composition of immigrant populations across local labor markets, we compute the 1910 foreign-born population share and its breakdown by origin region for each RLM and then group RLM–industry pairs into terciles of the exposure distribution. Table A4 summarizes these patterns. High-exposure RLM–industry pairs had both higher overall foreign-born shares and a much larger fraction of immigrants originating from high-restriction countries in Southern and Eastern Europe. In contrast, low-exposure markets had smaller foreign-born shares and immigrants more heavily drawn from low- or non-restriction origins such as Germany, Ireland, Scandinavia, and Canada.

Table A4. Composition of Foreign-Born Population by Exposure to Immigration Restrictions

Quota Exposure:	**Full Sample (1)**	**Bottom Tercile (2)**	**Middle Tercile (3)**	**Top Tercile (4)**
Number of RLM-Industry Pairs	399	133	133	133
Quota Exposure Measure (RLM-Industry):	0.109	0.022	0.101	0.204
Foreign-Born Share in Population (1910):	0.171	0.055	0.177	0.282
A. High-Restriction Countries:	34%	28%	31%	43%
Asia	2.3%	2.9%	1.6%	2.3%
Central Europe	9.8%	5.2%	10.5%	13.5%
Eastern Europe	0.4%	0.3%	0.5%	0.4%
Greece	1.2%	1.8%	1.0%	0.8%
Italy	8.6%	6.7%	7.3%	11.7%
Portugal	0.3%	0.1%	0.1%	0.7%
Rest of World	1.2%	1.4%	1.0%	1.0%
Russia	10.1%	9.1%	9.1%	12.2%
Spain	0.2%	0.2%	0.1%	0.3%
B. Low-Restriction Countries:	53%	57%	56%	47%
Germany	20.9%	22.9%	24.5%	15.2%
Ireland	8.9%	8.4%	7.2%	11.2%
Scandinavia	7.3%	4.9%	9.5%	7.6%
United Kingdom	12.2%	15.9%	10.1%	10.5%
Western Europe	3.9%	4.7%	4.5%	2.5%
C. Non-Restriction Countries:	13%	16%	13%	10%
Canada	10.7%	11.7%	11.5%	8.8%
Caribbean	0.4%	0.5%	0.1%	0.5%
Latin America	0.1%	0.1%	0.0%	0.0%
Mexico	1.7%	3.2%	1.2%	0.7%

Notes: The table reports the composition of the foreign-born population across RLM–industry pairs in 1910, grouped by terciles of the quota exposure measure QE_{ij}. For each group we show the mean quota exposure, the overall foreign-born share in the population, and the distribution of foreign-born by origin region. Percentages in Panels A–C sum to 100 within each column and refer to shares of the foreign-born population.

B Theoretical Framework and Derivations

This appendix presents more details on the formal model used to interpret the empirical results in the main text. The framework is partial equilibrium: local markets take the rental rate of capital and product prices as given, and capital and labor responses are modeled at the industry–labor market-cell level. A full general-equilibrium model with endogenous national factor prices, product markets, and complete inter-regional migration would require additional assumptions that extend beyond the scope of our empirical analysis. Because our empirical strategy exploits variation in exposure across industries and local labor markets within a national economy, the partial-equilibrium framework provides an appropriate basis for interpreting the wage effects we estimate.

B.1 Economic Environment

Consider local labor market–industry cells indexed by (i, j). Output is produced using capital K_{ij}, low-skilled labor $L_{1,ij}$, and skilled labor $L_{2,ij}$:

$$Q_{ij} = \Big[(1-\alpha_{ij})K_{ij}^{\delta_{ij}} + \alpha_{ij}L_{ij}^{\delta_{ij}}\Big]^{1/\delta_{ij}}, \qquad L_{ij} = \Big(\theta_{1,ij}L_{1,ij}^{\beta_{ij}} + \theta_{2,ij}L_{2,ij}^{\beta_{ij}}\Big)^{1/\beta_{ij}}, \tag{11}$$

where industry parameters $\alpha_i \in (0,1)$ and $\theta_{1,i}, \theta_{2,i} > 0$ govern factor shares and skill weights. The elasticities of substitution are

$$\sigma_{KL,ij} = \frac{1}{1-\delta_{ij}}, \qquad \sigma_{12,ij} = \frac{1}{1-\beta_{ij}}.$$

Firms are perfectly competitive and maximize profits

$$\Pi_{ij} = Q_{ij} - w_{1,ij}L_{1,ij} - w_{2,ij}L_{2,ij} - rK_{ij}, \tag{12}$$

implying standard factor-pricing first-order conditions:

$$w_{k,ij} = \frac{\partial Q_{ij}}{\partial L_{k,ij}}, \quad k = 1,2, \qquad r = \frac{\partial Q_{ij}}{\partial K_{ij}}. \tag{13}$$

We treat the rental rate of capital r as fixed to reflect access to the national capital market.

B.2 Cost Shares and Log-Linearized Factor Demand

Let total factor payments be

$$Y_{ij} = w_{1,ij}L_{1,ij} + w_{2,ij}L_{2,ij} + rK_{ij}. \tag{14}$$

Define cost shares:

$$S_{K,ij} = \frac{rK_{ij}}{Y_{ij}}, \qquad S_{L,ij} = 1 - S_{K,ij}, \qquad S_{1,ij} = \frac{w_{1,ij}L_{1,ij}}{w_{1,ij}L_{1,ij} + w_{2,ij}L_{2,ij}}, \quad S_{2,ij} = S_{L,ij} - S_{1,ij}. \tag{15}$$

A log-linear approximation to the first-order conditions around the initial equilibrium yields

$$d \ln w_{k,ij} = \varepsilon_{k1,ij}\, d \ln L_{1,ij} + \varepsilon_{k2,ij}\, d \ln L_{2,ij} + \varepsilon_{kK,ij}\, d \ln K_{ij}, \quad k = 1, 2, \tag{16}$$

where the coefficients depend on $\sigma_{KL,ij}, \sigma_{12,ij}$ and the cost shares $S_{K,ij}, S_{L,ij}, S_{1,ij}, S_{2,ij}$. These parameters vary across industries, generating heterogeneous wage responses to a common shock.

B.3 Short-Run Effects (Fixed Capital)

In the short run, the capital stock is fixed:

$$d \ln K_{ij} = 0. \tag{17}$$

Differentiation of (13) yields, for low-skilled wages,

$$\begin{aligned} d \ln w_{1,ij} = {} & \frac{1}{\sigma_{KL,ij}\sigma_{12,ij}} \left[\sigma_{12,ij} \Big((S_{L,ij} - 1) \tfrac{S_{1,ij}}{S_{L,ij}} \Big) + \sigma_{KL,i} \Big(\tfrac{S_{1,ij}}{S_{L,ij}} - 1 \Big) \right] d \ln L_{1,ij} \\ & + \frac{1}{\sigma_{KL,ij}\sigma_{12,ij}} \left[\sigma_{12,ij} \Big((S_{L,ij} - 1) \tfrac{S_{2,ij}}{S_{L,ij}} \Big) + \sigma_{KL,ij} \tfrac{S_{2,ij}}{S_{L,ij}} \right] d \ln L_{2,ij}, \end{aligned} \tag{18}$$

and an analogous expression for $d \ln w_{2,ij}$.

The relative wage response is independent of capital–labor substitution:

$$d \ln w_{1,ij} - d \ln w_{2,ij} = \frac{1}{\sigma_{12,ij}} \big(d \ln L_{2,ij} - d \ln L_{1,ij} \big). \tag{19}$$

A larger proportional decline in low-skilled labor $(d \ln L_{1,ij} < d \ln L_{2,ij})$ implies an unambiguous increase in $w_{1,ij}/w_{2,ij}$, with magnitude governed by $\sigma_{12,ij}$.

B.4 Medium-Run Effects: Partial Adjustment of Capital

Over the medium run, firms adjust capital in response to changes in labor supply. We model partial adjustment as

$$d \ln K_{ij}^{(m)} = \gamma_{ij}\, d \ln K_{ij}^{*}, \qquad 0 \le \gamma_{ij} \le 1, \tag{20}$$

where $d \ln K_{ij}^{*}$ is the long-run desired change in capital, and γ_{ij} measures the degree of adjustment. Industry differences in capital intensity, replacement costs, and the feasibility of shifting technologies imply heterogeneity in γ_{ij}.

Substituting (20) into the log-linear demand system yields the own-wage elasticity in the medium run:

$$\left. \frac{\partial \ln w_{k,ij}}{\partial \ln L_{k,ij}} \right|_{m} = \frac{1}{\sigma_{KL,ij}\sigma_{12,ij}} \left[\sigma_{12,ij} \left((S_{K,ij}\gamma_{ij} + S_{L,ij} - 1) \frac{S_{k,ij}}{S_{L,ij}} \right) + \sigma_{KL,ij} \left(\frac{S_{k,ij}}{S_{L,ij}} - 1 \right) \right], \quad k = 1, 2. \tag{21}$$

When $\gamma_{ij} = 0$, this reduces to the short-run elasticity. As γ_{ij} increases, the term $(S_{K,ij}\gamma_{ij} + S_{L,ij} - 1)$ increases, making the own-wage elasticity less negative and attenuating the wage response to a given contraction in labor supply. Industries with slow capital adjustment (small γ_{ij}) thus exhibit larger and more persistent wage increases.

B.5 Labor Mobility

Local labor supply may respond to wage changes through internal migration or labor-force entry. We model labor-supply responses as

$$d\ln L_{k,ij} = \pi_{k,ij} + \rho_{k,ij}\, d\ln w_{k,ij}, \qquad k = 1, 2, \tag{22}$$

where $\pi_{k,ij} < 0$ represents the exogenous decline in labor supply due to immigration restrictions, and $\rho_{k,ij}$ is the semi-elasticity of local labor supply with respect to wages.

To make the interaction between demand and supply explicit, it is useful to write the short-run demand system in a compact form. Let

$$d\ln w_{1,ij} = a_{11,ij}\, d\ln L_{1,ij} + a_{12,ij}\, d\ln L_{2,ij}, \qquad d\ln w_{2,ij} = a_{21,ij}\, d\ln L_{1,ij} + a_{22,ij}\, d\ln L_{2,ij}, \tag{23}$$

where the $a_{kl,ij}$ are the relevant elasticities implied by (18) and its analogue for $w_{2,ij}$.[42]

Case 1: Only Low-Skilled Labor Is Mobile. To illustrate the mechanism clearly, consider first the case in which only low-skilled labor responds to wages (i.e., $\rho_{1,ij} > 0$ and $\rho_{2,ij} = 0$). Then:

$$d\ln L_{1,ij} = \pi_{1,ij} + \rho_{1,ij}\, d\ln w_{1,ij}, \qquad d\ln L_{2,ij} = \pi_{2,ij}. \tag{24}$$

Substituting (24) into the first equation of (23) gives

$$d\ln w_{1,ij} = a_{11,ij}(\pi_{1,ij} + \rho_{1,ij} d\ln w_{1,ij}) + a_{12,ij}\pi_{2,ij}.$$

Rearranging,

$$(1 - a_{11,ij}\rho_{1,ij})\, d\ln w_{1,ij} = a_{11,ij}\pi_{1,ij} + a_{12,ij}\pi_{2,ij}.$$

Hence the equilibrium wage response for low-skilled workers is

$$d\ln w_{1,ij} = \frac{a_{11,ij}\pi_{1,ij} + a_{12,ij}\pi_{2,ij}}{1 - a_{11,ij}\rho_{1,ij}}. \tag{25}$$

The denominator $(1 - a_{11,ij}\rho_{1,ij})$ captures the amplifying or dampening role of labor mobility. Since $a_{11,ij} < 0$ (downward-sloping labor demand) and $\rho_{1,ij} \geq 0$, the denominator is larger than one in

[42] For example, $a_{11,ij}$ is the short-run own-wage elasticity of low-skilled labor demand with respect to low-skilled labor in cell (i, j), and $a_{12,ij}$ is the cross-elasticity with respect to skilled labor. Their exact expressions in terms of $\sigma_{KL,ij}, \sigma_{12,ij}$ and cost shares follow from (18).

absolute value, implying

$$|d \ln w_{1,ij}| \leq |a_{11,ij}\pi_{1,ij} + a_{12,ij}\pi_{2,ij}|$$

with strict inequality when $\rho_{1,ij} > 0$. When labor is immobile ($\rho_{1,ij} = 0$), (25) collapses to the pure demand-side response. As $\rho_{1,ij}$ increases, migration and labor-force entry offset the initial contraction, reducing the local wage effect.

The response of skilled wages follows from the second equation of (23):

$$\begin{aligned} d \ln w_{2,ij} &= a_{21,ij}(\pi_{1,ij} + \rho_{1,ij} d \ln w_{1,ij}) + a_{22,ij}\pi_{2,ij} \\ &= a_{21,ij}\pi_{1,ij} + a_{22,ij}\pi_{2,ij} + a_{21,ij}\rho_{1,ij} \frac{a_{11,ij}\pi_{1,ij} + a_{12,ij}\pi_{2,ij}}{1 - a_{11,ij}\rho_{1,ij}}. \end{aligned} \tag{26}$$

Thus both $d \ln w_{1,ij}$ and $d \ln w_{2,ij}$ are attenuated by labor mobility, but the magnitude of the effect depends on the interaction of own- and cross-price elasticities.

Case 2: Both Skill Groups Are Mobile. In the more general case, let $d \ln L_{ij}$ and $d \ln w_{ij}$ denote the 2×1 vectors of quantity and wage changes. Combining the demand system (23) and supply responses (22) yields

$$d \ln w_{ij} = A_{ij}\, d \ln L_{ij}, \qquad d \ln L_{ij} = \pi_{ij} + R_{ij}\, d \ln w_{ij}, \tag{27}$$

where

$$A_{ij} = \begin{bmatrix} a_{11,ij} & a_{12,ij} \\ a_{21,ij} & a_{22,ij} \end{bmatrix}, \qquad R_{ij} = \begin{bmatrix} \rho_{1,ij} & 0 \\ 0 & \rho_{2,ij} \end{bmatrix}, \qquad \pi_{ij} = \begin{bmatrix} \pi_{1,ij} \\ \pi_{2,ij} \end{bmatrix}.$$

Substituting and solving,

$$d \ln w_{ij} = A_{ij}\big(\pi_{ij} + R_{ij} d \ln w_{ij}\big) \quad \Rightarrow \quad (I - A_{ij}R_{ij})\, d \ln w_{ij} = A_{ij}\pi_{ij}. \tag{28}$$

Provided $(I - A_{ij}R_{ij})$ is nonsingular, the equilibrium wage response is

$$d \ln w_{ij} = (I - A_{ij}R_{ij})^{-1} A_{ij}\pi_{ij}. \tag{29}$$

This expression makes clear that labor mobility attenuates wage responses: when $R_{ij} = 0$ (no mobility), $d \ln w_{ij} = A_{ij}\pi_{ij}$; as R_{ij} increases, the matrix $(I - A_{ij}R_{ij})^{-1}$ moves wage responses closer to zero by making local labor supply more elastic.

Historical evidence indicates that labor mobility among low-skilled workers in the early twentieth century was limited, implying relatively small $\rho_{k,ij}$ and thus limited attenuation of the wage effects. This is consistent with the persistent wage differentials we document and with the internal migration patterns shown in Figure 14 and Table C5.

B.6 Long-Run Equilibrium

In the long run, capital and labor have fully adjusted. Capital stocks reach their desired levels and internal migration arbitrages most purely local wage differences. However, the immigration quotas permanently reduce the relative supply of low-skilled labor, which affects the long-run skill premium.

The key object is the labor nest in (11):

$$L_{ij} = \left(\theta_{1,ij} L_{1,ij}^{\beta_{ij}} + \theta_{2,ij} L_{2,ij}^{\beta_{ij}}\right)^{1/\beta_{ij}}.$$

Because Q_{ij} depends on $L_{1,ij}$ and $L_{2,ij}$ only through L_{ij}, the ratio of wages equals the ratio of marginal products of the labor aggregate:

$$\frac{w_{1,ij}}{w_{2,ij}} = \frac{\partial Q_{ij}/\partial L_{1,ij}}{\partial Q_{ij}/\partial L_{2,ij}} = \frac{\partial L_{ij}/\partial L_{1,ij}}{\partial L_{ij}/\partial L_{2,ij}}.$$

Differentiating L_{ij},

$$\frac{\partial L_{ij}}{\partial L_{1,ij}} = \theta_{1,ij} L_{1,ij}^{\beta_{ij}-1} L_{ij}^{1-\beta_{ij}}, \qquad \frac{\partial L_{ij}}{\partial L_{2,ij}} = \theta_{2,ij} L_{2,ij}^{\beta_{ij}-1} L_{ij}^{1-\beta_{ij}}.$$

Thus

$$\frac{w_{1,ij}}{w_{2,ij}} = \frac{\theta_{1,ij} L_{1,ij}^{\beta_{ij}-1}}{\theta_{2,ij} L_{2,ij}^{\beta_{ij}-1}} = \left(\frac{\theta_{1,ij}}{\theta_{2,ij}}\right)\left(\frac{L_{2,ij}}{L_{1,ij}}\right)^{1-\beta_{ij}}. \tag{30}$$

Using the relationship $\sigma_{12,ij} = 1/(1-\beta_{ij})$, we have $1-\beta_{ij} = 1/\sigma_{12,ij}$, so

$$\frac{w_{1,ij}}{w_{2,ij}} = \left(\frac{\theta_{1,ij}}{\theta_{2,ij}}\right)\left(\frac{L_{2,ij}}{L_{1,ij}}\right)^{1/\sigma_{12,ij}}. \tag{31}$$

Let $(\bar{w}_{1,ij}, \bar{w}_{2,ij})$ and $(\bar{L}_{1,ij}, \bar{L}_{2,ij})$ denote long-run equilibrium wages and quantities. Then

$$\frac{\bar{w}_{1,ij}}{\bar{w}_{2,ij}} = \left(\frac{\theta_{1,ij}}{\theta_{2,ij}}\right)\left(\frac{\bar{L}_{2,ij}}{\bar{L}_{1,ij}}\right)^{1/\sigma_{12,ij}}. \tag{32}$$

This expression shows that, even after full adjustment of capital and labor, the steady-state skill premium depends on the long-run relative supplies of low-skilled and skilled labor. If immigration restrictions permanently reduce $\bar{L}_{1,ij}$ relative to $\bar{L}_{2,ij}$, the ratio $(\bar{L}_{2,ij}/\bar{L}_{1,ij})$ rises, and the skill premium $\bar{w}_{2,ij}/\bar{w}_{1,ij}$ falls correspondingly.[43]

[43] The exact magnitude depends on the elasticity of substitution $\sigma_{12,ij}$. A higher $\sigma_{12,ij}$ (greater substitutability) implies a weaker effect of relative quantities on relative wages.

B.7 Industry Heterogeneity

All elasticities depend on $\sigma_{KL,ij}, \sigma_{12,ij}$ and the cost shares $(S_{K,ij}, S_{L,ij}, S_{1,ij}, S_{2,ij})$, which vary by industry and region. Two implications are central for our empirical results:

1. Sectors with high capital intensity and large fixed-capital stocks face higher adjustment costs and lower γ_{ij}, generating larger and more persistent wage effects (e.g., manufacturing and mining).

2. Sectors with modular capital or readily available mechanization paths adjust faster (higher γ_{ij}), yielding more muted wage responses (e.g., certain agricultural and light-industrial activities).

B.8 Summary

The model yields three core results:

1. In the short run, a disproportionate contraction in low-skilled labor raises low-skilled wages and compresses the skill premium, with the magnitude governed by $\sigma_{12,ij}$.

2. In the medium run, capital adjustment and limited labor mobility attenuate wage responses, with slower capital adjustment (lower γ_{ij}) producing larger and more persistent wage increases in some industries.

3. In the long run, the permanent reduction in low-skilled labor leaves a lasting imprint on the wage structure through (32), even after full adjustment of capital and labor.

C Additional Results

Additional Results - Immigration and Employment.

Table C1. Exposure to Border Closure Policy and Laborers Employment - by Skill

Sample: Pooled	**Overall (1)**	**Laborers (2)**	**Other Low-Skill (3)**	**Medium-Skill (4)**	**High-Skill (5)**
A. Recent Immigrants Share:					
Quota Exposure X Post 1920	-0.508***	-0.929***	-0.390***	-0.200***	-0.115***
	(0.0331)	(0.0545)	(0.0270)	(0.0172)	(0.0180)
Pre-1920 Dependent Mean	0.09	0.15	0.07	0.05	0.03
B. Log Overall Employment:					
Quota Exposure X Post 1920	-0.759***	-0.980***	-1.234***	-0.845***	-0.318
	(0.212)	(0.254)	(0.252)	(0.218)	(0.197)
Pre-1920 Dependent Mean	6,021	1,732	1,214	2,639	388
Quota Exposure - Mean	0.11	0.11	0.11	0.11	0.11
Quota Exposure - SD	0.08	0.08	0.08	0.08	0.08
Number of RLMs	125	125	125	125	125
Number of Industries	18	18	18	18	18
Number of RLM-Industry Pairs	399	399	399	399	399
Observations (RLM-Industry Pair)	1,197	1,197	1,197	1,197	1,197

Notes: This table reports coefficients on the interaction between the quota exposure measure and a post-1920 indicator for the pooled sample. Columns 1–5 report the log number of workers in different occupation categories within each RLM-industry pair: overall employment (column 1); laborers in column 2; other low-skill workers (service workers, farmers, and operatives) in column 3; medium-skill workers (crafts, clerical, and sales) in column 4; and high-skill workers (managers and professionals) in column 5. Panel A shows results for recent immigrant arrivals (within 10 years) from quota-restricted countries; Panel B shows results for total employment. All outcomes are measured at the Relevant Labor Market (RLM) by industry level. All specifications include RLM-by-industry and industry-by-year fixed effects. Each RLM-industry pair appears in the 1910, 1920, and 1930 censuses. Robust standard errors, clustered at the RLM-by-industry level, are shown in parentheses. Significance levels are indicated as *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C2. Exposure to Border Closure Policy and Laborers Employment - by Subgroup

Sample:	**Pooled**	**Manufacturing & Mining**	**American Contractor**	**Construction Union**	**Agriculture**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
1. (Log) Overall Employment (Laborers):					
Quota Exposure X Post 1920	-1.047***	-1.294***	-1.356*	-0.0712	0.360
	(0.279)	(0.360)	(0.734)	(0.488)	(0.449)
Pre-1920 Dependent Mean	1,732	1,409	2,210	3,148	1,490
2. Recent Immigrants - Quota Restricted Countries:					
Quota Exposure X Post 1920	-0.942***	-1.116***	-0.774***	-0.674***	-0.547***
	(0.0528)	(0.0597)	(0.115)	(0.149)	(0.0476)
Pre-1920 Dependent Mean	0.15	0.17	0.14	0.16	0.05
3. Earlier Immigrants - Quota Restricted Countries:					
Quota Exposure X Post 1920	0.262***	0.332***	0.163*	0.217**	0.0902
	(0.0380)	(0.0501)	(0.0910)	(0.102)	(0.0723)
Pre-1920 Dependent Mean	0.17	0.18	0.17	0.23	0.08
4. Immigrants - Non-Quota Countries:					
Quota Exposure X Post 1920	0.102***	0.153***	0.0236	0.0257	0.0712
	(0.0246)	(0.0401)	(0.0209)	(0.0369)	(0.0900)
Pre-1920 Dependent Mean	0.04	0.05	0.01	0.03	0.03
5. Native Born - Whites:					
Quota Exposure X Post 1920	0.379***	0.488***	0.358**	0.0610	0.134
	(0.0597)	(0.0657)	(0.175)	(0.113)	(0.140)
Pre-1920 Dependent Mean	0.47	0.46	0.41	0.37	0.68
6. Native Born - Non-Whites:					
Quota Exposure X Post 1920	0.198***	0.143***	0.229*	0.370***	0.252***
	(0.0433)	(0.0496)	(0.126)	(0.112)	(0.0692)
Pre-1920 Dependent Mean	0.17	0.14	0.27	0.21	0.17
Quota Exposure - Mean	0.11	0.11	0.11	0.13	0.07
Quota Exposure - SD	0.08	0.08	0.10	0.09	0.06
Number of RLMs	125	48	58	45	48
Number of Industries	18	16	1	1	1
Number of RLM-Industry Pairs	400	249	58	45	48
Observations (RLM-Industry Pair)	1,197	744	174	135	144

Notes: This table reports coefficients on the interaction between the quota exposure measure and a post-1920 indicator. Column (1) uses the pooled sample; columns (2)–(5) present results by data source. The outcome variable is the log number of laborers (OCC1950 codes 810–979) employed within each RLM-industry pair in row (1) and the employment share of a group in rows 2-6. Results are reported for six population groups, in the following order: overall, recent immigrant arrivals from quota-restricted countries, earlier immigrants from quota-restricted countries, immigrants from non-quota countries, native-born whites, and native-born non-whites. All outcomes are measured at the Relevant Labor Market (RLM) by industry level. Specifications include RLM-by-industry and industry-by-year fixed effects. Each RLM-industry pair appears in the 1910, 1920, and 1930 censuses. Robust standard errors, clustered at the RLM-by-industry level, are reported in parentheses. Significance levels are indicated as *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Additional Results - Semi-Skilled Workers.

Table C3. Exposure to Border Closure Policy and Construction Workers' Wages

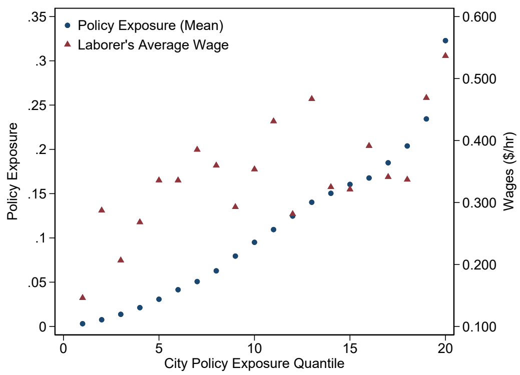
Sample:	**American Contractor**		
Occupation (Construction Industry):	**Laborers**	**Hod-Carriers**	**Bricklayers**
	(1)	**(2)**	**(3)**
A. Foreign Born Share:			
1. *All Foreign-Born*			
Quota Exposure X Post 1920	-0.591***	-0.366*	-0.0639
	(0.150)	(0.208)	(0.0589)
Pre-1920 Dependent Mean	0.32	0.05	0.28
2. *Quota Countries*			
Quota Exposure X Post 1920	-0.615***	-0.178	-0.0762
	(0.145)	(0.117)	(0.0626)
Pre-1920 Dependent Mean	0.30	0.04	0.26
B. Log Mean Hourly Wage:			
Quota Exposure X Post 1920	0.665***	0.214	0.0858
	(0.205)	(0.224)	(0.139)
1920 Mean Hourly Wage	0.614	0.757	1.234
Quota Exposure - Mean		0.13	
Quota Exposure - SD		0.10	
Number of Cities	54	49	55
Number of States	26	26	27
Observations (City-Year Pairs)	288	279	298

Notes: This table presents estimation results using the American Contractor survey sample which reports mean wages of various construction occupations during the 1920s. Column 1 shows estimates for construction laborers (low-skilled), column 2 for hod-carriers (medium-skill), and colum 3 for bricklayers (skilled). The outcomes in Panel A are the foreign-born share (overall and from quota restricted countries) for the occupation in the city. The outcomes in Panel B are the (log) mean hourly wage for workers in a given occupation in a city. The table presents the coefficients of the interaction between quota exposure measure and the post-1920 indicator. Foreign-Born shares are calculated as the share of foreign-born workers in the construction industry at the county level using the full count population censuses of 1910,1920, and 1930. Wage data is at the city-year level and is obtained from the American Contractor surveys of construction contractors in the 1920s. Each observation in the regression represents a city-year pair. All specifications include year and city fixed effects. Robust standard errors, clustered at the city level, are in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

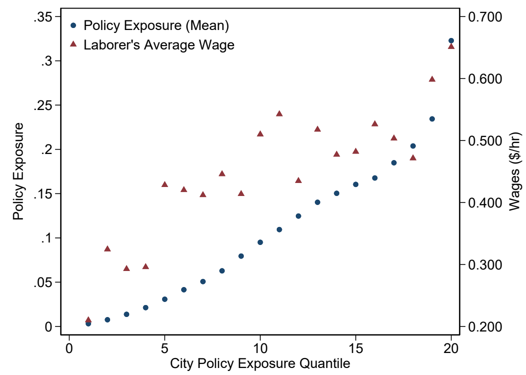
Additional Results - Municipal Street Laborers.

Figure C1. Cross-Sectional Exposure to Border Closure Policy and Wages - Pooled Sample

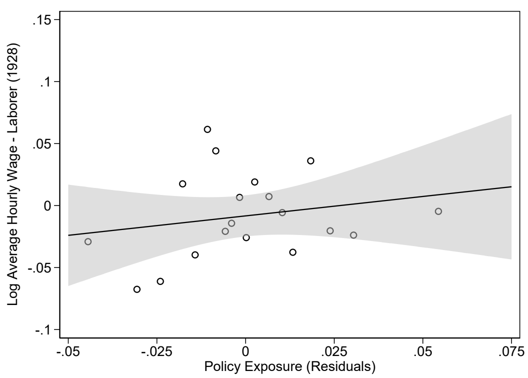
(a) Unadjusted: Pre-1920



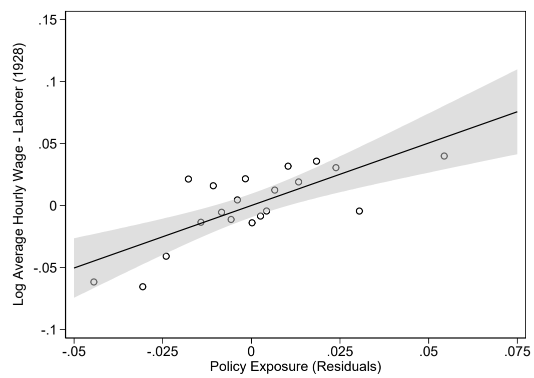
(b) Unadjusted: Post-1920



(c) Residualized: Pre-1920



(d) Residualized: Post-1920



Notes: Panels (a) and (b) plot the relationship between RLM-industry policy exposure quantile and policy exposure mean (blue) and mean hourly wage pre- and post-1920 (red triangles), respectively, for each quantile. Each of the 399 RLM-industry pairs from the pooled sample is assigned into quantiles. Panels (c) and (d) plot the relationship between policy exposure and log mean hourly wage pre- and post-1920 using a binned scatter plot. We exclude outliers with wage residuals above .2 or below -.1. Each RLM-industry policy exposure and wages are adjusted for the set of controls listed in Table D4, and state fixed effects. The red line presents the fitted linear relationship between the residuals of policy exposure and log mean hourly wages. The dashed gray lines are 95% confidence intervals.

Table C4. Municipal Street Laborers Workers Wage Data - Summary Statistics

	Mean Exposure (1)	**Wage (1928)** (2)	**Cities** (3)	**Mean Size** (4)	**Largest City** (5)
Full Sample	0.10	$0.43	2,295	25,216	New York, NY (6.1m)
Exposure Measure Quartile:					
Quartile 1 (Bottom)	0.01	$0.31	576	9,257	Decatur, IL (157k)
Quartile 2	0.04	$0.40	574	20,087	Davenport, IA (552k)
Quartile 3	0.12	$0.49	574	23,868	St. Louis, MO (848k)
Quartile 4 (Top)	0.25	$0.52	571	47,827	New York, NY (6.1m)

Notes: Columns 1-5 show the mean border closure exposure measure, mean hourly municipal laborer wage in 1928, number of cities/towns, mean city population, and the name of the largest city for the full sample and by its exposure quartile of cities and towns, respectively.

Additional Results - Mechanisms

Table C5. Composition Change and Internal Migration by Quota Exposure

Sample:	**DID Estimate** (1)	**Std. Error** (2)	**Pre-1920 Mean** (3)
A. Entries (not in sample in T and in sample in T+10):			
Overall	-0.001	(0.025)	0.88
Remained Laborer	0.032	(0.033)	0.287
Remained Laborer & Move to Higher QE	0.022	(0.029)	0.157
Became Laborer	-0.042	(0.035)	0.300
Enter LF	0.027	(0.037)	0.298
B. Exits (in Sample in T but not in T+10):			
Overall	-0.104***	(0.019)	0.887
Remained Laborer	0.007	(0.035)	0.265
Remained Laborer & Move to Higher QE	0.004	(0.026)	0.106
Change Occupation from Laborer	-0.156***	(0.048)	0.528
Exit LF	0.094	(0.057)	0.097
RLM-industry Pairs		399	
Observations		798	

Notes: This table presents difference-in-differences estimates of the interaction between quota exposure (QE) and a post-1920 indicator. Column 1 shows the DID estimate, column 2 shows robust standard errors clustered at the RLM-industry level, and column 3 shows pre-1920 means. Panel A examines entries (not in sample at T, in sample at T+10): overall entries, remained laborer, remained laborer and moved to higher QE, became laborer, and entered labor force. Panel B examines exits (in sample at T, not in sample at T+10): overall exits, remained laborer, remained laborer and moved to higher QE, changed occupation from laborer, and exited labor force. All specifications include year and RLM-industry fixed effects, dataset- and industry-specific linear time trends, and RLM-industry controls interacted with linear time trends. Sample includes 399 RLM-industry pairs observed 1910-1930 (798 observations). Robust standard errors, clustered at the RLM-by-industry level, are reported in parentheses. Significance levels are indicated as *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

D Robustness

This appendix presents the full set of robustness analyses summarized in subsection 6.4. We report results from specifications using alternative definitions of exposure, additional covariates, different aggregation levels, and controls for wartime disruptions.Tables D1–D8 show that the estimated post-1920 wage effects are stable across all exercises. The coefficients remain positive and statistically significant when exposure is defined at the state level, when using binary or 1900-based measures, and when the data are aggregated to the RLM or industry level. Including a broad set of pre-1910 demographic and industrial covariates has little effect on the estimates, indicating that the results are not driven by omitted pre-existing differences. Finally, incorporating a measure of World War I immigration exposure yields an insignificant coefficient, confirming that the wage responses reflect the impact of the 1920s immigration restrictions rather than temporary wartime shocks.

Table D1. Pre Border Closure Policy Trends in Wages

Sample:	**Pooled Sample**		**Manufacturing & Mining**		**Construction Union**		**Farm Wages**	
Trend Type:	**Linear**	**Non-Linear**	**Linear**	**Non-Linear**	**Linear**	**Non-Linear**	**Linear**	**Non-Linear**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Quota Exposure X Linear Trend	-0.004		-0.044		0.015		0.000	
	(0.033)		(0.055)		(0.035)		(0.020)	
Quota Exposure X Year 1911-12		-0.073		0.202		-0.057		-0.378**
		(0.168)		(0.298)		(0.127)		(0.146)
Quota Exposure X Year 1913-14		0.010		0.197		0.443		0.039
		(0.176)		(0.339)		(0.393)		(0.106)
Quota Exposure X Year 1915-16		0.213		0.481		0.250		0.169
		(0.206)		(0.297)		(0.481)		(0.172)
Quota Exposure X Year 1917-18		-0.053		-0.374		0.582		-0.053
		(0.276)		(0.475)		(0.461)		(0.164)
Quota Exposure X Year 1919-20		-0.377		-0.781		1.041**		-0.261
		(0.302)		(0.495)		(0.421)		(0.205)
R-Squared	0.587	0.663	0.636	0.753	0.537	0.695	0.620	0.707
Region Real Wage - Mean	0.258		0.245		0.397		0.186	
Region Quota Exposure - Mean	0.100		0.107		0.152		0.066	
Region Quota Exposure - SD	0.082		0.079		0.098		0.059	
Observations	1,353		550		241		528	

Notes: The table presents the results of two tests for the existence of pre-trends in log mean hourly wages in the pooled sample (columns 1-2), BLS industry surveys of manufacturing and mining industries (columns 3-4), the BLS surveys of construction unions in selected cities (columns 5-6), and USDA survey of agriculture labor wages (columns 7-8). The years included in all specifications are the pre-policy years 1910-1920. The table presents the interaction coefficient for each sample between quota exposure and a linear time trend in one specification and the coefficients between the quota exposure measure and year dummies in another specification. The omitted year is 1910. The specification also includes year dummies and quota exposure measures. Robust standard errors in parenthesis are clustered at the level of the relevant region by industry. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

Pre-Trends. To assess whether log mean hourly wages exhibited similar pre-1920 trends across RLM–industry pairs with different exposure levels, we restrict the sample to 1910–1920 and regress wages on the exposure measure, a linear time trend, and their interaction (Table D1, col. 1). We then allow for nonlinear pre-policy dynamics by replacing the linear trend with year dummies for 1911–1920 (Table D1, col. 2). In neither specification do we find significant evidence that pre-1921 wage trends varied with exposure. Repeating these tests separately by dataset yields the same conclusion.

Alternative Exposure Measures. We test the robustness of our findings to alternative definitions of the quota exposure measure in Table D2. Column 1 reports our baseline specification, where exposure is defined at the RLM–industry level by applying county–industry employment shares within each state to the county-level exposure measure as defined in Equation 8. To address possible concerns with use of country employment shares in creating RLM exposure measures, we construct a state-level exposure measure that ignores the RLM definition and aggregates exposure at the state level. Formally, this is the analogous of Equation 7,

$$q_s = \sum_r \frac{FB_{rs1910}}{LF_{s1910}} \times QI_r \tag{33}$$

where FB_{rs1910} and LF_{s1910} denote the number of foreign-born and total working-age (16–65) male workers in state s in 1910, respectively. For the construction datasets, we retain the county-level exposure q_c, as these measures are directly aligned with the underlying data.

Column (2) of Table D2 reports estimates using this state-based exposure. The results for both recent immigrant shares and wages remain statistically significant and similar in magnitude to our preferred RLM–industry specification. Furthermore, Table D3 and Figure D1 shows that event study estimates using a state-based exposure measure produces very similar results to our preferred RLM-based exposure measure event studies. This confirms that our results are not driven by the additional variation created when assigning distinct exposure measures at the geography–industry level rather than at the geographic level alone. In column (3), we further modify the country-specific quota intensity variable QI_r, replacing the continuous counterfactual measure with a binary indicator equal to one for high-restriction countries and zero otherwise. The results remain robust,.

Finally, columns (4)–(6) of Table D2 we use the 1900 Census instead of 1910 to construct both the RLM-based and state-based measures (including the binary specification). The results remain virtually unchanged,.

Table D2. Robustness to Alternative Quota Exposure Measures

	1910 Census			1900 Census		
Exposure Measure:	**Baseline (RLM-Industry) (1)**	**State Based (2)**	**Binary Exposure (3)**	**RLM-Industry Based (4)**	**State Based (5)**	**Binary Exposure (6)**
A. Recent Immigrants Share:						
Quota Exposure X Post 1920	-0.511***	-0.390***	-0.492***	-0.606***	-0.520***	-0.601***
	(0.0334)	(0.0276)	(0.0335)	(0.0587)	(0.0519)	(0.0611)
Pre-1920 Dependent Mean	0.09	0.08	0.09	0.09	0.08	0.09
Quota Exposure - Mean	0.11	0.10	0.11	0.06	0.06	0.06
Quota Exposure - SD	0.08	0.08	0.09	0.05	0.05	0.05
Number of Panel Units	399	399	399	397	399	397
Observations	1,197	1,197	1,197	1,191	1,197	1,191
B. Log Mean Hourly Wage:						
Quota Exposure X Post 1920	0.615***	0.662***	0.675***	0.540**	0.643***	0.728***
	(0.148)	(0.135)	(0.128)	(0.252)	(0.246)	(0.249)
Pre-1920 Dependent Mean	0.258	0.258	0.258	0.258	0.258	0.258
Quota Exposure - Mean	0.100	0.105	0.104	0.059	0.062	0.058
Quota Exposure - SD	0.082	0.081	0.083	0.054	0.053	0.052
Number of Panel Units	399	399	399	397	399	399
Observations	2,973	2,973	2,973	2,965	2,973	2,973

Notes: This table reports the coefficients of the interaction between quota exposure measures and the post-1920 indicator. Panel A uses the share of recent immigrants from quota countries as the dependent variable, while Panel B uses log mean hourly wage. The post-1920 indicator equals one in years after 1920, the last census year before the quota policy took effect. Columns (1)–(3) construct quota exposure using 1910 Census, while Columns (4)–(6) use 1900 Census. Columns (1) and (4) use continuous industry-level quota exposure based on the RLM by Industry sample; Columns (2) and (5) use continuous state-level quota exposure; and Columns (3) and (6) use binary exposure indicators (quota countries foreign born share in 1900 or 1910). All specifications include year, industry, and RLM-industry or state-industry fixed effects, respectively. Standard errors, clustered at the RLM-industry or state-industry level, are reported in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

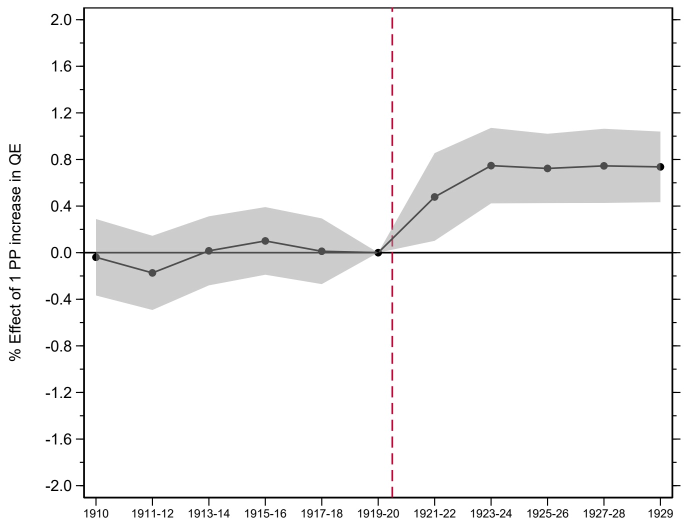
Table D3. Robustness to Alternative Quota Exposure Measures - Event Study Estimates

Exposure Measure:	1910 Census			1900 Census		
	Baseline (RLM-Industry) (1)	State Based (2)	Binary Exposure (3)	RLM-Industry Based (4)	State Based (5)	Binary Exposure (6)
Quota Exposure X Year 1910	-0.141	-0.0394	-0.0694	-0.0858	0.0559	-0.00890
	(0.184)	(0.167)	(0.165)	(0.279)	(0.264)	(0.282)
Quota Exposure X Year 1911-12	-0.257	-0.174	-0.192	-0.171	-0.0648	-0.103
	(0.173)	(0.162)	(0.160)	(0.265)	(0.259)	(0.275)
Quota Exposure X Year 1913-14	-0.0528	0.0158	-0.00701	0.0258	0.131	0.0924
	(0.163)	(0.151)	(0.148)	(0.244)	(0.233)	(0.242)
Quota Exposure X Year 1915-16	-0.0270	0.101	0.0991	-0.0149	0.142	0.158
	(0.159)	(0.148)	(0.146)	(0.239)	(0.225)	(0.233)
Quota Exposure X Year 1917-18	-0.0892	0.0121	-0.000348	-0.0628	0.0764	0.0636
	(0.158)	(0.144)	(0.141)	(0.225)	(0.209)	(0.215)
Quota Exposure X Year 1921-22	0.402**	0.478**	0.469**	0.546**	0.702***	0.732***
	(0.198)	(0.192)	(0.186)	(0.256)	(0.254)	(0.256)
Quota Exposure X Year 1923-24	0.642***	0.747***	0.747***	0.568**	0.746***	0.809***
	(0.172)	(0.165)	(0.159)	(0.267)	(0.272)	(0.271)
Quota Exposure X Year 1925-26	0.579***	0.723***	0.735***	0.454*	0.686**	0.785***
	(0.163)	(0.151)	(0.146)	(0.267)	(0.266)	(0.264)
Quota Exposure X Year 1927-28	0.639***	0.745***	0.759***	0.599**	0.789***	0.895***
	(0.174)	(0.162)	(0.157)	(0.275)	(0.274)	(0.276)
Quota Exposure X Year 1929	0.615***	0.736***	0.744***	0.544**	0.778***	0.876***
	(0.173)	(0.154)	(0.149)	(0.255)	(0.252)	(0.254)
Pre-1920 Dependent Mean	0.258	0.258	0.258	0.258	0.258	0.258
Quota Exposure - Mean	0.100	0.105	0.104	0.059	0.062	0.058
Quota Exposure - SD	0.082	0.081	0.083	0.054	0.053	0.052
Number of Panel Units	399	399	399	397	399	399
Observations	2,973	2,973	2,973	2,965	2,973	2,973

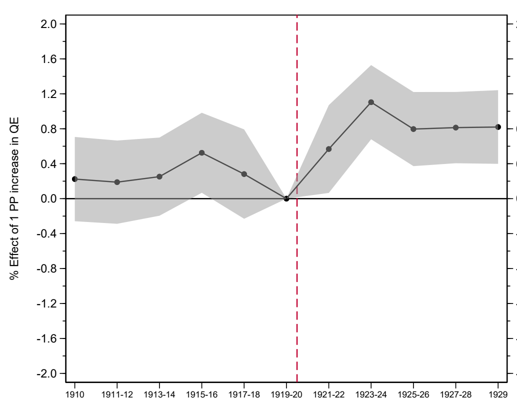
Notes: This table reports the coefficients of the interaction between quota exposure measures and year coefficients. The outcome variable is RLM-industry log mean hourly wage. The year indicators are grouped in pairs to except for 1910 and 1929. Columns (1)–(3) construct quota exposure using 1910 Census, while Columns (4)–(6) use 1900 Census. Columns (1) and (4) use continuous industry-level quota exposure based on the RLM by Industry sample; Columns (2) and (5) use continuous state-level quota exposure; and Columns (3) and (6) use binary exposure indicators (quota countries foreign born share in 1900 or 1910). All specifications include year, industry, and RLM-industry or state-industry fixed effects, respectively. Standard errors, clustered at the RLM-industry or state-industry level, are reported in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

Figure D1. Event-Study Estimates - State Based Measure

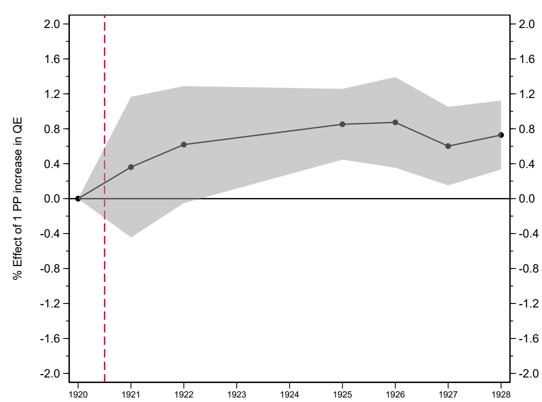
(a) Pooled Sample



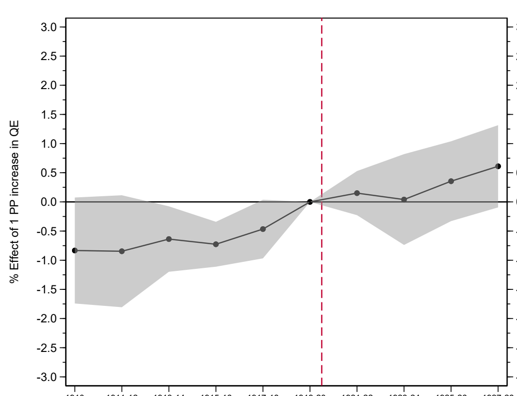
(b) Manufacturing & Mining



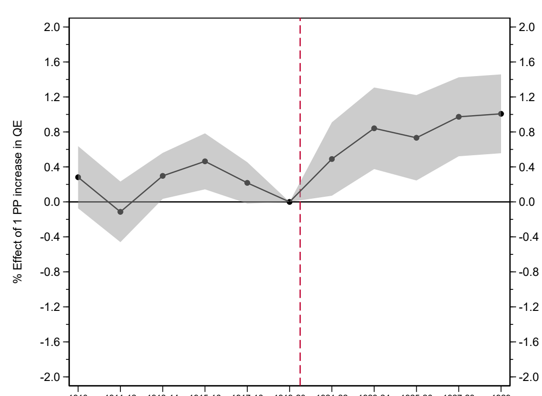
(c) American Contractor



(d) Construction Union



(e) Farm Labor



Notes: The figures plot the event-study coefficients of the interaction between exposure to border closure policy measure and year from Equation 10 for each of the four datasets used in our analysis separately using the state-level data and measures. All specifications include year and RLM by industry fixed effects. Each RLM-industry pair has a varying number of observations from 1910-1930. The gray area shows 95% confidence intervals. Standard errors are clustered at the RLM by industry level.

Inclusion of Covariates. To assess the robustness of our results to the inclusion of economic and demographic controls, we examine the relationship between RLM–industry exposure and a broad set of 1910 covariates across datasets. As shown in Table D4, a Lasso procedure identifies three key predictors of quota exposure in the pooled sample: the 1910 foreign-born population share, the log of total population, and the share of workers in agriculture. Following Abramitzky et al. (2023), we also include the 1910 foreign-born share directly in our wage regressions to account for potential "scale effects" of immigration restrictions and isolate the impact driven by differences in immigrant composition.

Table D5 and Table D6 report estimates from specifications that add all Lasso-selected 1910 controls (and the foreign-born share for wage regressions) interacted either with a linear time trend or with a post-1920 indicator.[44] The pooled difference-in-differences estimates in Table D5 show that adding these controls leaves the main results unchanged. For wage regressions in Panel B, the inclusion of covariates does not affect the pooled estimates materially.

Table D6 presents the corresponding event-study estimates with covariates included. The pre-1920 coefficients remain statistically insignificant and the post-1920 effects stay positive and similar in magnitude to the baseline. Overall, these results indicate that our findings are robust to controlling for pre-existing demographic and economic differences across local labor markets, and that the estimated effects are not driven by omitted variation in 1910 characteristics.

[44]Interacting 1910 controls with full year fixed effects is too demanding given the limited number of observations, particularly in the event-study regressions. Allowing post-1920 trend changes based on these covariates provides a more parsimonious way to capture differential dynamics.

Table D4. Lasso Results for the Relationship Between Exposure to Border Closure Policy and 1910 Region Characteristics

Sample:	**Pooled**	**Manufacturing & Mining**	**American Contractor**	**Construction Union**	**Agriculture**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
A. Demographics:					
Log Total Population	x	x	x	x	-
Share Urban Population	-	-	-	-	-
Share Black Population	x	x	x	x	-
Literacy Rate	-	-	-	-	-
B. Occupation:					
Share Workers in Blue-Collar Occupations (Manufacturing)	-	-	-	-	-
Share Workers in Farming Occupations (Agriculture)	x	x	x	x	-
Share Workers Holding White Collar Occupation	-	-	-	-	-
C. Industry:					
Share Workers in Mining	-	-	-	-	-
Share Workers in Construction	-	-	-	-	x
Share Workers in Transportation	-	-	-	-	-
Share Workers in Wholesale/Retail	-	-	-	-	x
Share Workers in Services	-	-	-	-	-
Share Workers in Public Administration	-	-	-	-	-
C. Farming:					
Log Average Farm Value	-	x	-	-	-
Log Value of Farm Output per Acre	-	-	-	-	-
Share Owner Operated Farms	-	-	-	-	-
Share Farmland Cultivated	-	-	-	-	-
Share Wheat in Cultivated Farmland	-	-	-	-	-
Share Cotton in Cultivated Farmland	-	-	-	-	-
Share Hay/Corn in Cultivated Farmland	-	-	-	-	-
Region Quota Exposure - Mean	0.109	0.113	0.111	0.129	0.066
Region Quota Exposure - SD	0.083	0.081	0.096	0.087	0.059
Number of Regions	399	248	58	45	48

Notes: Columns 1-5 show the variables selected by a lasso procedure of a cross-sectional specification where the dependent variable is the Relevant Labor Market (RLM) by industry exposure measure and the potential explanatory variables are a set of 1910 RLM by industry characteristics. Controls marked with an "x" are chosen by the Lasso specification.

Table D5. Robustness to Inclusion of Covariates

Sample:	Pooled	Manufacturing & Mining	American Contractor	Construction Union	Agriculture
	(1)	(2)	(3)	(4)	(5)
A. Recent Immigrants Share:					
Baseline Specification	-0.511***	-0.667***	-0.308***	-0.259***	-0.351***
	(0.0334)	(0.0404)	(0.0465)	(0.0546)	(0.0383)
Covariates: All Lasso Selected Controls (Linear Trends)	-0.305***	-0.342***	-0.211***	-0.0428	-0.171**
	(0.0421)	(0.0599)	(0.0703)	(0.0758)	(0.0670)
Covariates: All Lasso Selected Controls X Post 1920 (Non-Linear Trend)	-0.501***	-0.684***	-0.322***	-0.194**	-0.378***
	(0.0477)	(0.0733)	(0.0550)	(0.0758)	(0.0564)
Pre-1920 Dependent Mean	0.09	0.11	0.06	0.08	0.03
B. Log Mean Hourly Wage:					
Baseline Specification	0.615***	0.541***	0.665***	0.767***	0.527
	(0.148)	(0.147)	(0.205)	(0.275)	(0.339)
Covariates: All Lasso Selected Controls (Linear Time Trends)	0.575***	0.620***	0.551*	1.064***	0.264
	(0.168)	(0.215)	(0.309)	(0.338)	(0.331)
Covariates: All Lasso Selected Controls X Post 1920 (Non-Linear Trend)	0.609**	0.443	0.729	2.206***	0.272
	(0.281)	(0.305)	(0.591)	(0.294)	(0.540)
Pre-1920 Dependent Mean	0.258	0.245	0.614	0.397	0.186
RLM-Industry Quota Exposure - Mean	0.11	0.11	0.11	0.13	0.07
RLM-Industry Quota Exposure - SD	0.08	0.08	0.10	0.09	0.06
Number of RLMs	125	48	58	45	48
Number of Industries	18	16	1	1	1
Number of RLM-Industry Pairs	399	248	58	45	48
Observations (RLM-Industry Pair)	1,197	744	174	135	144

Notes: This table reports robustness checks for the baseline specifications by including additional covariates. Panel A shows results for recent immigrant arrivals (within 10 years) from quota-restricted countries; Panel B shows results for log mean hourly wage. Coefficients correspond to the interaction between the quota exposure measure and a post-policy indicator. All outcomes are measured at the Relevant Labor Market (RLM) by industry level. Columns 1–5 report estimates for the pooled sample (col. 1), manufacturing & mining (col. 2), American contractor (col. 3), construction unions (col. 4), and agriculture (col. 5). Baseline specifications are shown in the first row of each panel. The next two rows add all Lasso-selected controls (see Table D4 for details), allowing either linear or non-linear trends. For wage outcome, we control for 1910 foreign-born share in addition. All specifications include RLM-by-industry and industry-by-year fixed effects. Robust standard errors, clustered at the RLM-by-industry level, are shown in parentheses. Significance levels are indicated as *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table D6. Robustness to Inclusion of Covariates - Event Studies

Covariates Included:	**Baseline (RLM-Industry Based)** **(1)**	**Lasso - Linear** **(2)**	**Lasso - Non-Linear** **(3)**
Quota Exposure X Year 1910	-0.141	-0.137	-0.0327
	(0.184)	(0.302)	(0.167)
Quota Exposure X Year 1911-12	-0.257	-0.258	-0.162
	(0.173)	(0.265)	(0.164)
Quota Exposure X Year 1913-14	-0.0528	-0.0664	0.0115
	(0.163)	(0.222)	(0.157)
Quota Exposure X Year 1915-16	-0.0270	0.0510	0.0909
	(0.159)	(0.183)	(0.149)
Quota Exposure X Year 1917-18	-0.0892	-0.00869	0.00407
	(0.158)	(0.156)	(0.140)
Quota Exposure X Year 1921-22	0.402**	0.492**	0.263
	(0.198)	(0.203)	(0.349)
Quota Exposure X Year 1923-24	0.642***	0.799***	0.538*
	(0.172)	(0.184)	(0.307)
Quota Exposure X Year 1925-26	0.579***	0.782***	0.502
	(0.163)	(0.209)	(0.311)
Quota Exposure X Year 1927-28	0.639***	0.834***	0.533*
	(0.174)	(0.243)	(0.316)
Quota Exposure X Year 1929	0.615***	0.777***	0.474
	(0.173)	(0.275)	(0.301)
Pre-1920 Dependent Mean	0.258	0.258	0.258
Quota Exposure - Mean	0.100	0.105	0.104
Quota Exposure - SD	0.082	0.081	0.083
Number of Panel Units	399	399	399
Observations	2,973	2,973	2,973

Notes: This table reports event study estimates of the interaction between quota exposure and year indicators, using log mean hourly wage at the RLM–industry level as the dependent variable. Year indicators are grouped in two-year intervals, except for 1910 and 1929, which are reported separately. Column (1) reports baseline estimates including RLM–industry fixed effects. Columns (2) and (3) add all Lasso-selected controls (see Table D4) and 1910 foreign-born share, allowing for linear trends (col. 2) or post-1920 interactions (col. 3). All specifications include RLM–industry and industry-by-year fixed effects. Robust standard errors, clustered at the RLM–industry level, are shown in parentheses. Significance levels are indicated as *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Aggregation of Labor Markets. A potential concern with our definition of a labor market as a geography–industry (RLM–industry) pair is that it assumes both geographic and inter-industry differences in exposure to immigration restrictions generated similar wage dynamics. Yet, low-skilled workers may have been able to move easily between industries within the same geographic area, implying that wage equalization across industries within a region could occur more rapidly than wage equalization across regions with differing exposure levels. In this case, our baseline specification—which treats each geography–industry cell as a separate labor market—could overstate within-region variation in exposure and understate the degree of adjustment through worker mobility.

To evaluate this possibility, we collapse the RLM–industry data to the geographic labor market (jurisdiction) level. Specifically, we construct an exposure measure for each RLM assuming no within-region variation across industries and compute the employment-weighted mean wage across all industries in that RLM and year. As reported in Table D7, higher exposure to immigration restrictions is associated with a decline in the recent-immigrant share and an increase in mean real wages for low-skilled workers. Both effects are slightly smaller than in our baseline estimates (Table 2 and Table 4), suggesting that within-region wage equalization across industries was limited and that our main specification does not overstate inter-industry adjustment.

Column (3) of Table D7 instead aggregates the data by industry rather than by labor market, testing whether our findings are primarily driven by exposure differences across industries rather than across locations. The results show that both the decline in immigrant shares and the increase in wages are larger than in the RLM–industry estimates.

Taken together, these aggregation exercises confirm that our main results are not sensitive to the level of analysis. Whether exposure is defined at the RLM–industry, RLM, or industry level, more exposed markets consistently exhibit lower immigrant employment shares and higher wages, underscoring the robustness of the wage effects of the 1920s immigration restrictions.

Table D7. Robustness of Estimates to Aggregation: RLM- and Industry-Level

Aggregation:	**RLM-Industry (1)**	**RLM (2)**	**Industry (3)**
A. Recent Immigrants Share:			
Quota Exposure X Post 1920	-0.511***	-0.416***	-0.854***
	(0.0334)	(0.0450)	(0.224)
Pre-1920 Dependent Mean	0.089	0.070	0.169
Observations	1,197	375	54
B. Log Mean Hourly Wage:			
Quota Exposure X Post 1920	0.388***	0.355***	0.423***
	(0.112)	(0.0964)	(0.140)
Pre-1920 Dependent Mean	0.344	0.360	0.837
Observations	852	303	39
Quota Exposure - Mean	0.106	0.116	0.443
Quota Exposure - SD	0.083	0.132	0.233
Number of Panel Units	399	125	18

Notes: This table reports coefficients on the interaction between quota exposure and a post-1920 indicator for different levels of sample aggregation. Column (1) has our baseline specification at the RLM-by-indutry level, column (2) is aggregated to the RLM level, and column (3) is aggreagted to the industry level. Panel A uses the share of recent immigrants from quota-restricted countries as the dependent variable. RLM–industry pairs are employment-weighted to construct measures at the RLM or industry level. Panel B uses log mean hourly wage as the dependent variable. RLM–industry pairs are first employment-weighted, and then the data are collapsed to the decadal level by assigning 1910–1915 to 1910, 1916–1925 to 1920, and 1926 onward to 1930. The regressions are weighted by the number of observations multiplied by RLM or industry-percent coverage for the period, which is the percent of overall 1910 employment that has a wage observation in the given year. All specifications include RLM-industry, RLM, or industry fixed effects, respectively. Clustered robust standard errors are in parenthesis. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

World War I. Because World War I preceded the introduction of the immigration quotas, disentangling the effects of wartime disruptions from those of the subsequent policy change is a central identification concern. Regions that relied heavily on European immigrants experienced large inflows before 1915, sharp wartime declines, and further reductions following the implementation of the quotas. This pattern raises the possibility that the observed wage and employment effects reflect temporary wartime shocks rather than the long-term consequences of the 1920s immigration restrictions.

To assess this possibility, we first turn to the event-study estimates presented earlier. These results show no systematic increase in wages during the war years. The pre-1920 coefficients are flat and statistically insignificant, indicating that wages in high- and low-exposure markets evolved similarly before the policy change. Wage effects emerge only after 1920, when the quota system came into force, and remain persistently positive throughout the decade. The absence of wage movements during the war period reinforces that our findings capture the causal impact of immigration restrictions rather than temporary disruptions in labor demand or supply related to wartime production and mobilization.

Table D8. Testing Robustness for Controlling for World War I Exposure

Exposure Measures:	**QE Exposure (1)**	**WWI Exposure (2)**	**QE + WWI Exposure (3)**
A. Recent Immigrants Share:			
Quota Exposure X Post 1920	-0.511***		-0.308***
	(0.0334)		(0.0800)
WWI Exposure X Post 1920		-0.345***	-0.148***
		(0.0198)	(0.0462)
Pre-1920 Dependent Mean		0.089	
Quota Exposure - Mean		0.109	
Quota Exposure - SD		0.084	
WW1 Exposure - Mean		0.183	
WW1 Exposure - SD		0.124	
Observations		1,197	
B. Log Mean Hourly Wages:			
Quota Exposure X Post 1920	0.615***		0.738***
	(0.148)		(0.146)
WWI Exposure X Post 1920		0.234**	-0.134
		(0.108)	(0.0887)
Pre-1920 Dependent Mean		0.258	
Quota Exposure - Mean		0.100	
Quota Exposure - SD		0.082	
WW1 Exposure - Mean		0.169	
WW1 Exposure - SD		0.124	
Observations		2,973	

Notes: The table presents the coefficient of the interaction between quota exposure QE and the post-policy change indicator. The Post variable is an indicator for post-1920. In Panel A, the dependent variable is the recent immigrants (10 years or less in US) employment share for the Relevant Labor Market (RLM) by industry pair. In Panel B, the dependent variable is the log mean hourly wage for Relevant Labor Market (RLM) by industry pair. All specifications include industry-year and RLM by industry fixed effects. Column 2 uses the WWI exposure measure instead of our preferred quota exposure measure. Column 3 includes both quota exposure and WWI exposure measures and their interactions. In Panel A, each RLM-industry pair has three observations for the 1910, 1920, and 1930 censuses. In Panel B, each RLM-industry pair has a different number of observations from 1910 to 1929 based on pooled sample data. Robust standard errors, clustered at the RLM-industry level, in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

We further test this interpretation using a more direct approach. Following Abramitzky et al. (2023), we construct a World War I exposure measure that captures the extent to which immigration flows were restricted by the war. We then re-estimate our main specification including both the quota and WWI exposure measures:

$$y_{ijt} = \delta_{ij} + \gamma_{it} + \beta(QE_{ij} \times 1(t \geq 1920)_t) + \theta(WE_{ij} \times 1(t \geq 1920)_t) + \varepsilon_{ijt} \tag{34}$$

Table D8 reports the results for the pooled sample. Column (1) presents our preferred baseline estimates, Column (2) includes only the WWI exposure measure, and Column (3) includes both quota and war exposure. When including only WWI exposure (Column 2), the estimated effects are smaller than those using quota exposure. In Column (3), when both exposures are included, the quota exposure coefficient remains positive and statistically significant, while the WWI exposure coefficient is smaller in magnitude and imprecisely estimated. For the recent-immigrant share, both exposure measures are significant, but the quota coefficient is roughly twice as large, indicating that although the war temporarily reduced immigration, the quotas imposed a more binding and persistent restriction. For wages, including the WWI measure increases the estimated quota effect, while the war coefficient is negative and insignificant, consistent with no systematic wage effects from wartime disruptions after 1920. Taken together, the absence of wage effects during the war years and the robustness of our results to explicit controls for WWI exposure provide further support that the observed wage and employment responses were driven mainly by the immigration policy change rather than by wartime shocks.

E Sample Selection and Composition Appendix

Possible SUTVA Violations. One of the assumptions required for the DID model to be interpreted as causal is the Stable Unit Treatment Value Assumption (SUTVA; see Angrist et al. (1996)). This assumption requires that there should be no changes in the composition of the treatment and control groups and that there is no spillover effect from the treatment of one unit on the outcome of another unit. However, as observed in prior studies by Abramitzky et al. (2023) and Price et al. (2020), the restrictions on immigration in the 1920s resulted in population composition changes in labor markets due to increased internal migration from low- to high-exposure areas. As a result, SUTVA could be violated in our context because the immigration policy restrictions indirectly affect labor markets that experience increased out-migration to more highly exposed labor markets.

We would note that first, we are claiming to identify the effect of immigration restrictions and disruptions on more-exposed markets relative to less-exposed markets, given the structure of interactions across relevant labor markets that determine equilibrium adjustments. Put another way, we are identifying wage dynamics in the 1920s in response both to the labor supply shocks directly caused by the quota laws and the movements of labor in response to the wage changes initially caused by those labor supply shocks. Second, if migration from low- to high-exposed markets pushed wages down in the latter while pulling them up in the former, our estimates would be downward-biased estimates of the initial, direct effect of the immigration reductions on wages.

Nevertheless, we test for this potential bias by conducting two robustness checks: omitting labor markets with low exposure to treatment and labor markets with high rates of out-migration to treated areas. The first test is based on the idea that the least exposed areas are potentially most likely to have experienced high out-migration to high-exposed areas. The second test is based on Abramitzky et al. (2023) calculations of out-migration rates to higher exposed labor markets between 1920 and 1930. They define a labor market as a State Economic Area (SEA), a collection of counties within a state that are economically integrated. For our purposes, we compute the mean out-migration rate to highly exposed areas across SEAs within a given state and use the state's mean out-migration rate as a proxy for the extent of changes in composition in each relevant labor market in our sample. We then exclude from the estimation sample areas that this measure indicates were likely exposed to larger composition changes and spillover effects.

Table E1 provides the results of these tests for our the pooled sample. The outcome in Panel A is the relevant labor market by industry foreign-born share (in each decennial census), and the outcome in Panel B is the log mean hourly wage for each relevant labor market by industry pair (annual observations). Column 1 shows our baseline estimates from column 1 of Table 2 for reference. In columns 2-4, we show our test results when excluding RLM-Industry pairs at the exposure measure's bottom 1st,5th, and 10th percentiles. The foreign-born share and wage estimates remain unchanged in any of the specifications. In columns 5 and 6 of Table E1, we show the results of excluding RLM-industry pairs with the highest 5 percent or 10 percent of values of our "out-migration to higher exposure markets" rate. In column 7, we exclude the state with the highest out-migration rate within each census division. Again, the coefficients are all statistically significant and are similar to the baseline estimate.

Table E1. Testing for Potential Bias from Potential SUTVA Violation

		Exclude Bottom X% Quota Exposure:			Exclude Top X% Out-migration Rate		
Sample:	**Baseline**	**X =1**	**X = 5**	**X = 10**	**X = 5**	**X = 10**	**Top state (Division)**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
A. Recent Immigrants Share:							
Quota Exposure X Post 1920	-0.511***	-0.513***	-0.511***	-0.486***	-0.498***	-0.477***	-0.494***
	(0.0334)	(0.0337)	(0.0352)	(0.0373)	(0.0330)	(0.0299)	(0.0303)
Pre-1920 Dependent Mean	0.089	28.473	29.655	31.196	27.732	26.927	25.739
Quota Exposure - Mean	0.109	0.110	0.115	0.121	0.108	0.106	0.101
Quota Exposure - SD	0.084	0.083	0.082	0.080	0.084	0.084	0.079
Number of Panel Units	399	395	379	359	384	366	312
Observations	1,197	1,185	1,137	1,077	1,152	1,098	936
B. Log Mean Hourly Wage:							
Quota Exposure X Post 1920	0.615***	0.612***	0.629***	0.640***	0.681***	0.675***	0.601***
	(0.148)	(0.149)	(0.160)	(0.170)	(0.144)	(0.139)	(0.161)
Pre-1920 Dependent Mean	0.258	0.260	0.273	0.278	0.256	0.253	0.248
Quota Exposure - Mean	0.100	0.101	0.110	0.116	0.099	0.098	0.091
Quota Exposure - SD	0.082	0.082	0.080	0.078	0.083	0.085	0.078
Number of Panel Units	399	395	379	359	384	366	312
Observations	2,973	2,920	2,723	2,588	2,844	2,697	2,459

Notes: This table presents the coefficient of the interaction between quota exposure QE and the post policy change indicator for the pooled sample. The Post variable is defined as an indicator for post 1920. In Panel A, the dependent variable in the specifications is the foreign-born share for the Relevant Labor Market (RLM) by Industry pairs, as described in the text. In Panel B, the dependent variable in the specifications is the log mean hourly wage for Relevant Labor Market (RLM) by industry pair. All specifications include industry-year and RLM fixed effects. Columns 2-4 exclude RLMs in the bottom 1,5, and 10 percentile of the QE distribution, respectively. Columns 5-7 use Abramitzky et al. (2023) out-migration rate to high exposure areas, aggregated to the state level, and exclude the states in the top 5, 10, and highest in division out-migration to high exposure areas, respectively. In Panel A, each RLM-Industry pair has four observations for 1910, 1920, 1930, and 1940 censuses. In Panel B, each RLM-Industry pair has a different number of observations based on pooled sample data. Robust standard errors, clustered at the RLM by industry level, in parenthesis. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

Sample Selection. We also examine whether changes in the composition of our analysis sample from the pre- to post-1920 period might bias our estimated wage effects. Specifically, we assess selection into the wage sample—that is, the likelihood of having a wage observation in our data—by comparing demographic characteristics of laborers in RLM–industry pairs included in our wage sample to those of laborers in RLM–industry pairs not observed in the wage data. These comparisons are constructed using the full-count population censuses for 1910, 1920, and 1930, which provide comprehensive information on worker demographics and occupational structure. We then compare differences in these characteristics across high- and low-exposure RLM–industry pairs to evaluate whether the sample composition changed differentially following the immigration restrictions.

Table E2 examines differences in demographic composition between the wage sample and the broader population of laborers, as well as between high- and low-exposure RLM–industry pairs, using data from the 1910, 1920, and 1930 full-count censuses. Columns (1)–(3) report pre-1920 means for laborers in RLM–industry pairs included in the wage sample, those excluded from the sample, and the adjusted differences between the two groups after controlling for industry and state fixed effects. On average, RLM–industry pairs included in our wage analysis are slightly more exposed to immigration restrictions than those excluded, with a mean quota exposure of 0.11 versus 0.09. Before 1920, wage-sample laborers were more likely to be foreign-born (recent immigrant share of 0.16 vs. 0.13 and early immigrant share of 0.16 vs. 0.14, adjusted differences of 3 and 2.1 percentage points, respectively) and more likely to reside in urban areas (urban household rate of 0.61 vs. 0.55, an adjusted difference of 7.4 percentage points). These pre-1920 differences indicate that the wage-sample RLM–industry pairs were selected primarily along foreign-born and urban characteristics.

Columns (3)–(5) of Table E2 report difference-in-differences (DID) estimates that control for both industry and state fixed effects, isolating changes in sample composition from broader sectoral or geographic trends. Column (4) captures the overall post-1920 trend in workforce composition across all laborers, while column (5) presents the DID estimates comparing how the composition of workers in the wage sample evolved relative to that overall trend. The results indicate a 2.2 percentage-point decline in the immigrant share among wage-sample observations, suggesting that immigration restrictions disproportionately affected in-sample RLM–industry pairs relative to out-of-sample ones. At the same time, we observe a 1.8 percentage-point increase in the share of Black workers and a 1.9 percentage-point decline in the share of White workers, while the share of young workers aged 16–25 declined by about 1.7 percentage points. Lastly, while the initial difference in urban rates between in-sample and out-of-sample observations is significant, this gap remains stable post-1920, suggesting no composition change driven by urban status.

We next compare high- versus low-exposure RLM–industry pairs within the wage sample in columns (6)–(10) of Table E2, allowing us to assess whether composition shifted differentially across RLM–industry pairs with varying exposure to immigration restrictions. By construction, the difference in mean quota exposure between these groups is large (0.17 vs. 0.04): high-exposure RLM–industries are those above the median exposure value, while low-exposure pairs fall below it. Pre-1920 differences confirm that these groups were distinct even before the quota laws. High-exposure RLM–industry pairs had substantially higher recent and early immigrant shares (0.26 and 0.25, respectively, versus 0.06 and 0.07 in low-exposure cells) and were more urban (0.73 vs. 0.49). These contrasts reflect the pre-existing concentration of foreign-born labor in industrial and urban centers most affected by the quotas.

Post-1920, the DID estimates reveal sharp compositional adjustments. The share of recent immigrants fell dramatically in high-exposure areas—by 14.3 percentage points—while the Black share increased by 2.6 percentage points and the average age rose by 0.83 years. These changes are consistent with a shift away from foreign-born workers toward older, native-born, and more racially diverse labor forces in regions most affected by the restrictions. The rise in Black worker shares is less concerning from the standpoint of bias, as Black workers typically earned lower wages during this period—implying that this shift would attenuate, rather than inflate, our estimated wage effects. Similarly, the modest increase in mean age is unlikely to meaningfully affect wages,. Consequently, the primary source of potential mechanical bias remains the sharp decline in immigrant shares in high-exposure labor markets.

Table E2. Changes in Sample Composition and Demographic Characteristics Across Wage Sample and Exposure Groups

Sample Split:	Wage Sample vs. Not in Sample					High vs. Low Exposure				
	Pre-1920 Means		DID Results			Pre-1920 Means		DID Results		
	Not in-Sample	In-Sample	In-Sample Indicator	Post-1920 Indicator	DID Estimate	Low-Exposure	High-Exposure	High-Exposure Indicator	Post-1920 Indicator	DID Estimate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mean Quota Exposure	0.09	0.11				0.04	0.17			
Recent Immigrant (< 10 Years in U.S.)	0.13	0.16	0.030***	-0.093***	-0.022**	0.06	0.26	0.146***	-0.044***	-0.143***
			(0.007)	(0.010)	(0.009)			(0.013)	(0.007)	(0.010)
Early Immigrant (10+ Years in U.S.)	0.14	0.16	0.021***	0.006	0.012*	0.07	0.25	0.064***	-0.005	0.047***
			(0.007)	(0.006)	(0.007)			(0.013)	(0.004)	(0.010)
Black Indicator	0.19	0.16	0.003	-0.009	0.018***	0.27	0.05	-0.002	-0.004	0.026***
			(0.007)	(0.006)	(0.006)			(0.016)	(0.008)	(0.008)
White Indicator	0.81	0.84	-0.002	0.009	-0.019***	0.73	0.95	-0.003	0.004	-0.026***
			(0.008)	(0.006)	(0.006)			(0.017)	(0.008)	(0.008)
Urban Household	0.55	0.61	0.074***	0.020**	-0.016	0.49	0.73	0.106***	0.015	-0.021
			(0.016)	(0.010)	(0.012)			(0.017)	(0.011)	(0.013)
Age	33.8	33.2	-0.199	1.084***	0.369*	32.2	34.1	-0.210	1.038***	0.830***
			(0.145)	(0.188)	(0.206)			(0.236)	(0.133)	(0.169)
Age 16 to 25 Indicator	0.33	0.35	0.002	-0.009	-0.017***	0.40	0.31	-0.023***	-0.029***	0.006
			(0.004)	(0.005)	(0.006)			(0.008)	(0.005)	(0.006)
Single Indicator	0.44	0.47	-0.001	-0.071***	0.008	0.47	0.47	0.008	-0.064***	0.003
			(0.005)	(0.007)	(0.007)			(0.010)	(0.006)	(0.009)
Head of Household Indicator	0.49	0.44	-0.005	0.065***	0.007	0.45	0.43	-0.040***	0.054***	0.036***
			(0.006)	(0.007)	(0.007)			(0.010)	(0.007)	(0.008)

Notes: Columns (1)–(2) report pre-1920 means for laborers in region–industry cells that are included in the wage sample analysis versus laborers in region-industry cells that are not included in the wage analysis sample. Columns (3)–(5) present coefficients from a difference-in-differences regression that adjusts for state and industry fixed effects. Columns (6)–(7) report pre-1920 means for laborers in region–industry cells with below-median (low exposure) versus above-median (high exposure) values of the quota exposure measure. Columns (8)–(10) present coefficients from a difference-in-differences regression that adjusts for state and industry fixed effects. Each region–industry pair appears in the 1910, 1920, and 1930 U.S. censuses. The sample is restricted to laborers. "Recent immigrant" refers to individuals in the U.S. for fewer than 10 years, while "Early immigrant" refers to those in the U.S. for 10 or more years. Urban status, household headship, and other demographic indicators are coded at the individual level from census data. Robust standard errors, clustered at the state level, in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

Guided by these findings, we conduct formal tests of sample selection and potential selection bias in our wage estimates, reported in Table E3. These exercises assess whether the initial differences and compositional changes we document in Table E2 translate into systematic differences in the likelihood of being included in the wage sample or in a high-exposure RLM–industry pair, and whether such differences bias the estimated effect of immigration restrictions on wages. We focus on two complementary approaches: (i) an inverse probability weighting (IPW) correction that re-weights the wage sample to match the full population of laborers, and (ii) a pseudo-Heckman selection model that explicitly models selection into the wage sample and into high-exposure.

The first set of estimates, shown in columns (2) and (5) of Table E3, apply inverse probability weights based on the estimated probability that a laborer in a given RLM–industry cell is observed in the wage sample. These probabilities are estimated using a random-forest model on the 1910–1930 full-count censuses, where the outcome is a binary indicator for being a laborer in a sample RLM–industry pair in year t (1910, 1920, or 1930), and zero otherwise. The predictors include the 1910 RLM–industry characteristics listed in Table D4, along with year, industry, state, and dataset fixed effects. Reweighting the wage data using these predicted inclusion probabilities leaves the estimated wage effects virtually unchanged, suggesting that sample selection—particularly differences in urbanization and immigrant presence—does not materially affect our treatment estimates.

Columns (3) and (7) of Table E3 report estimates from a pseudo-Heckman selection correction model, designed to account for potential unobserved factors driving inclusion in the wage sample. We refer to it as "pseudo" because, unlike a standard Heckman two-step model estimated at the individual level, our first-stage selection equation is estimated using individual-level data from the full-count censuses, while the second-stage wage regressions are estimated at the RLM–industry–year level. To bridge these levels of analysis, we aggregate the individual-level inverse Mills ratio (IMR) to its mean value within each RLM–industry–year cell before including it in the wage equation.

In the first stage, we estimate a probit model of selection into the wage sample at the RLM–industry level:

$$S^*_{prijt} = \beta \times QE_{ij} + \gamma^p \times X^p_{pijt} + \gamma^{RLM} \times X^{RLM}_{ij1910} + \delta_t + \pi_r + \omega_i + \varepsilon_{ijt} \tag{35}$$

where S^*_{prijt} is an indicator equal to one if individual p in census region r in RLM-industry pair ij in year t is a laborer in the wage sample and zero otherwise, QE_{ij} is RLM-industry ij exposure measure, X^p is a set of individual-level controls, X^{RLM} includes the 1910 RLM-industry characteristics listed in Table D4, and δ_t, π_r, and ω_i are year, region, and industry fixed effects, respectively.[45]

From this model, we compute the RLM-industry aggregated inverse Mills ratio (IMR), defined as

$$\bar{\lambda}_{ijt} = \frac{1}{N_{ijt}} \sum_{p \in ijt} \frac{\phi(\hat{S}_{prijt})}{\Phi(\hat{S}_{prijt})} \tag{36}$$

where N_{ij} is the number of individuals working as laborers in RLM-industry pair ij in year t, $\hat{S}_{prijt}$ is the fitted value from the probit model in Equation 35, $\phi(\cdot)$ and $\Phi(\cdot)$ denote the standard normal probability density function and cumulative distribution function, respectively. Intuitively, the inverse Mills ratio measures the expected value of the error term in the outcome equation conditional on being observed in the sample—capturing the extent to which unobserved factors

[45] Individual level controls include age, race, nativity, marital status, urban status, head of household indicator, currently in school indicator, and group quarters indicator.

jointly affect selection and outcomes. A significant coefficient on the IMR in the second stage would thus indicate that selection into the observed wage sample is non-random and correlated with unobserved determinants of wages.

Columns (3) and (7) of Table E3 show that the IMR coefficients are negative and statistically significant (–0.488 and –0.294, respectively), indicating negative selection into the wage sample: RLM–industry cells more likely to appear in the sample tend to have slightly lower unobserved wage components. Despite this, the inclusion of the IMR term leaves the main treatment effects largely unchanged, with the DID coefficient on quota exposure increasing only slightly, and the event-study coefficients remaining stable and free of pre-trend differences.

Columns (4) and (8) of Table E3 extend the pseudo-Heckman framework to examine selection into high-exposure RLM–industry pairs, using only individuals observed in the wage sample. The first-stage probit models the probability that a laborer belongs to a high-exposure RLM–industry pair (above the median quota exposure value),

$$H^*_{prijt} = \gamma^p \times X^p_{pijt} + \delta_t + \pi_r + \omega_i + \varepsilon_{ijt} \tag{37}$$

where H^*_{prijt} is an indicator equal to 1 if individual p is a laborer in RLM-industry pair ij with above median exposure in year t and zero if he is a laborer in an RLM-industry pair in year t with below median exposure. As before, we compute an individual-level inverse mills ratio and aggregate it to the RLM-industry-year level by averaging the individual-level observations.

As before, the resulting IMR is aggregated to the RLM–industry–year level and included in the wage regressions. The estimated IMR coefficients are negative (–0.367 in column 4 and –0.873 in column 8), with the latter statistically significant. These results indicate that RLM–industry cells more likely to be highly exposed to the quotas tend to have somewhat lower unobserved wage components. However, inclusion of this correction term has minimal impact on the key treatment effects: the DID coefficient on quota exposure and the event-study patterns remain robust, showing a persistent post-1920 divergence between high- and low-exposure RLMs and no evidence of differential pre-trends.

Overall, the results indicate that while selection into the wage sample and high-exposure markets is statistically detectable, it does not materially bias our estimated wage effects. The negative IMR coefficients suggest mild negative selection—RLM–industry cells more likely to appear in the wage sample tend to have slightly lower unobserved wage components—but accounting for this has little impact on the estimated treatment effects. If anything, the true wage response to immigration restrictions may be slightly understated.

Table E3. Robustness of Wage Effects to Sample Selection Models

Estimation Model:	Pooled DID Baseline (1)	Pooled DID IPW (2)	Pooled DID Pseudo Heckman Selection to Sample (3)	Pooled DID Pseudo Heckman Selection to Exposure (4)	Event Study Baseline (5)	Event Study IPW (6)	Event Study Pseudo Heckman Selection to Sample (7)	Event Study Pseudo Heckman Selection to Exposure (8)
Quota Exposure X Post 1920	0.615***	0.618***	0.655***	0.708***				
	(0.148)	(0.148)	(0.144)	(0.159)				
Inverse-Mills Ratio (IMR)			-0.479***	-0.367			-0.316**	-0.873***
			(0.152)	(0.257)			(0.151)	(0.317)
Quota Exposure X (Year = 1910)					-0.141	-0.135	-0.202	-0.387*
					(0.184)	(0.184)	(0.190)	(0.201)
Quota Exposure X (Year = 1911-1912)					-0.257	-0.251	-0.314*	-0.464**
					(0.173)	(0.173)	(0.177)	(0.190)
Quota Exposure X (Year = 1913-1914)					-0.0528	-0.0475	-0.0962	-0.210
					(0.163)	(0.164)	(0.165)	(0.173)
Quota Exposure X (Year = 1915-1916)					-0.0270	-0.0191	-0.0449	-0.125
					(0.159)	(0.160)	(0.159)	(0.157)
Quota Exposure X (Year = 1917-1918)					-0.0892	-0.0835	-0.0874	-0.131
					(0.158)	(0.158)	(0.159)	(0.157)
Quota Exposure X (Year = 1921-1922)					0.402**	0.417**	0.402**	0.454**
					(0.198)	(0.199)	(0.198)	(0.200)
Quota Exposure X (Year = 1923-1924)					0.642***	0.650***	0.646***	0.737***
					(0.172)	(0.172)	(0.173)	(0.177)
Quota Exposure X (Year = 1925-1926)					0.579***	0.585***	0.586***	0.727***
					(0.163)	(0.163)	(0.163)	(0.179)
Quota Exposure X (Year = 1927-1928)					0.639***	0.642***	0.653***	0.842***
					(0.174)	(0.174)	(0.174)	(0.200)
Quota Exposure X (Year = 1929)					0.615***	0.623***	0.623***	0.893***
					(0.173)	(0.172)	(0.172)	(0.205)
Pre-1920 Mean Hourly Wage ($)	0.258	0.258	0.258	0.258	0.258	0.258	0.258	0.258
Number of RLM-Industry Pairs	399	399	399	399	45	48	48	48
Observations (RLM-Industry Pair)	2,973	2,973	2,973	2,973	487	960	960	960

Notes: This table reports robustness checks addressing potential non-random selection into the wage sample. The outcome variable is the log mean hourly wage of low-skilled laborers by region–industry cell. Columns 1-4 present pooled difference-in-differences estimates, where the coefficient on Quota Exposure × Post-1920 captures the average impact of immigration restrictions after 1920. Columns 5-8 report event-study estimates by census year, relative to the 1919-1920 baseline. Columns (1) and (5) present the baseline specification, which includes year and region–industry fixed effects as well as industry-by-year fixed effects. Columns (2) and (6) reweight observations using inverse probability weights (IPW) constructed from the first-stage probability of observing wage data in a region-industry cell using 1910 region-industry characteristics, thereby correcting for differential sample selection. Columns (3) and (7) and (4) and (8) report estimates from a pseudo-Heckman approach that augments the DID regression with the Inverse Mills Ratio (IMR) constructed from the first-stage probit of selection into the wage sample (high exposure) on quota exposure, individual level demographics, and 1910 region-industry characteristics, in addition to census division, 1-digit industry, and time fixed effects. The IMR at the region-industry level is the mean of the individual IMR's obtained from the first-stage. Standard errors, clustered at the region–industry level, are reported in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

Immigrant Wage Penalty. In this section, we describe in more detail our assessment of the extent to which the wage effects we observe are driven by the falling immigrant share among laborers in the more exposed RLM-industry pairs, following the imposition of quotas.

As described in the text, we produce estimates of the native immigrant wage differential from three data sources. The first is the report of the US Immigration Commission or Dillingham Commission, established by Congress in 1907 to conduct an investigation into "the subject of immigration". One aspect of the commission's investigation was the collection of employment and earnings data in over 20 industries that employed a disproportionate number of immigrants. Information on over half a million immigrant and native-born workers was obtained from employer payroll records of 1907 (USIC,1911). To measure the immigrant-native wage differential for each industry, we use the reported industry-wide ratio of the average wage for foreign-born male employees to the average wage for native-born workers. The commission reported data for 8 of the 21 industries used in our analysis. According to the commission's data, the immigrant wage penalty in those 8 industries was 9.3% on average, and it ranged from 25% in the construction industry to a wage premium of about 5% in metal mining.

One caveat of the Dillingham Commission wage data is that they report industry-wide wage differentials rather than low-skilled labor wage differentials, which might imply an inflated wage penalty since immigrants were more likely to be concentrated in low-skilled and low-wage occupations during this period. Thus, we complement our analysis with data from the 1915 Iowa State Census Project, which provides one of the only individual-level earnings datasets available before 1920 (Goldin and Katz, 2000). The census covers a representative sample of Iowa's population and includes information on age, nativity, education, occupation, and annual income. Although confined to a single state, the Iowa data allow us to estimate the immigrant wage penalty using pre-1920 individual-level wage observations.

After restricting the sample to men aged 16–65 working as laborers with reported annual earnings, we estimate regressions of the form:

$$ln(w_{ic}) = \beta immig_i + \Gamma X_i + \gamma_c + \varepsilon_{ic} \tag{38}$$

where $ln(w_{ij})$ is the log annual earnings of individual i residing in county c, $immig_i$ is an indicator equal to one if individual i is foreign-born arriving at the US within the last 10 years (recent immigrant), X_i is a set of additional individual-level covariates, and γ_c are county fixed-effects.[46] Although industry information is unavailable, we estimate separate models for farm laborers and coal-mine laborers, allowing partial differentiation across broad sectors.

Our Iowa estimates indicate an immigrant wage penalty of 7.1 percentage points on average. The adjusted penalties for farm laborers and coal miners are slightly larger, around 9 percentage points. Overall, these Iowa results align closely with the nationwide estimates from the Dillingham Commission and provide a valuable pre-1920 benchmark confirming that immigrants systematically earned lower wages than natives.

Despite that, neither the Dillingham Commission nor the Iowa State Census report wage differentials for all the industries covered in our analysis, so we also estimate the immigrant wage penalty using the 1940 Full Count Population Census, the first Census to report actual wages. Despite being reported many years after the restrictions went into effect, using the full count Census

[46]The additional covariates include an indicator for early-immigrant status (more than 10 years in the US), age and age-squared, indicators for marital status (single, married, divorced, widowed, missing), race indicators (white, black, other), and education attainment categories (no education, common school. grammar school, high school, and college or more).

allows us to estimate wage penalties for all the industries in our data. We construct a sample of working-age males aged 16-65 with a reported laborer occupation in one of the 21 industries used in our analysis. Then, we estimate the following regression model for the sample of low-skilled workers:

$$ln(w_{ijc}) = \sum_j \beta_j immig_{ijc} + \Gamma X_i + \gamma j + \delta_c + \varepsilon_{ij} \tag{39}$$

where $ln(w_{ijc})$ is the log hourly wage of individual i in industry j in county c, β_j are industry indicators, $immig_{ijc}$ is a recent immigrant indicator (arrived within the last 5 years), X_i is a set of additional individual-level covariates, γ_j are industry fixed effects, and δ_c are county fixed effects.[47]

Our estimates range from 5 percent in the agriculture sector to as large as 70 percent in the shoes industry. The aggregated estimate from the 1940 Census is 9 percent.[48]

Assessing the Composition Effects Channel The main text describes how we use the approach of Ager et al. (2024) to evaluate the extent to which the observed post-1920 wage increases might reflect mechanical composition effects arising from the sharp decline in immigrant shares.

Panel (a) of Figure 13 shows the distribution of the percent of total wage effect that is due to composition shift across RLM-industry pairs when assuming a constant 10 percent wage penalty. The thick dashed red line presents the median percent, estimated at slightly less than 10 percent, and the thin dotted red lines show the 25th and 75th percentiles of the composition effect. Most RLM-industry pairs have a very low estimated composition effect, suggesting that composition shifts are not the primary explanation for the estimated wage effects in most RLM-industry pairs. Table E4 presents the table analogue of panel (a) of Figure 13 when assuming a constant 10 percent immigrant wage penalty. In the median RLM–industry pair, composition effects explain 6.2 to 9.3 percent of total wage changes, depending on whether we use recent-immigrant or overall foreign-born shares. Over two-thirds of pairs exhibit effects below 10 percent, and more than 90 percent fall below 25 percent.

[47]The additional covariates include an indicator for early-immigrant status (more than 5 years in the US), age and age-squared, indicator single status, head of household status, urban household, farm household, race indicators (white, black, hispanic, other), and five education attainment categories.

[48]We also have another specification where we control for individual demographics, including age and age-squared, place of birth, marital status, education, and race. The estimated immigrant wage penalty is 5.9% in that specification.

Table E4. Composition Effects Estimates and Distribution Across RLM-Industry Pairs

	DID Estimate	**DID - Wage**	**Comp. Effect (%, Median)**	**Composition Effect**			
				$< 10\%$	**$< 15\%$**	**$< 25\%$**	**$\geq 25\%$**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Recent Immigrants Share - Quota Countries	-0.511***	0.615***	6.22	209	247	285	19
	(0.0333)	(0.148)		{69%}	{81%}	{94%}	{6%}
Foreign-Born Share	-0.300***	0.615***	9.25	161	225	276	28
	(0.0322)	(0.148)		{53%}	{74%}	{91%}	{9%}

Notes: This table reports results from our calculations of what share of the predicted increase in hourly wages due to the immigration quotas is accounted for directly by changes in the workforce share of recent immigrants from quota restricted countries or foreign-born workers in general. Changes in wages are calculated from 1910-1920, before the quotas, to 1930, the first post-quota years with census data on the foreign-born share of the population. We report the median share of wages accounted for by composition (column 3), as well as the number of RLM-industry pairs (share in curly brackets) where composition accounts for less than 10% (column 4), less than 15% (column 5), less than 25% (column 6) of wages, as well as the cities where composition changes account for 25% or more of the hourly wage change from pre-1920 to 1930. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.